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CLINICAL RESEARCH IN PSYCHIATRY*

By

G. A. FOULDS, M.A., Ph.D.
Runwell Hospital, Wickford, Essex

INTRODUCTION

SINCE the author has been working for some fourteen years alongside psychiatric colleagues his slender assets are as heavily and inextricably invested in the future of Psychiatry as those of any psychiatrist. Such criticisms as are offered must, therefore, from this point of view be regarded as within rather than between group criticisms.

To anticipate the charge of flogging a dead horse or setting up a straw-man, the literature on the assessment of the effects of treatment was examined for the years 1951 to 1956 inclusive. Thirty-six papers were culled from the British literature (*J. Ment. Sci.*, 31; *J. Neurol. and Neuropsychiat.*, 5) and 36 from the American (*J. Nerv. and Ment. Dis.*, 7; *Amer. J. Psychiat.*, 20; *Psychiat. Qtrly.*, 9). Alternate copies of each journal were examined. If the appropriate copy was not available, the next one back was taken.

In the British literature 16, or 44 per cent., of the studies used control groups of some sort. These were not always the appropriate ones and occasionally the author seems to have lost control, as it were, somewhere between the Introduction and the Results. The author claimed that the treatment was a success in 19 per cent. of the studies in which controls were used; on the other hand, 85 per cent. of authors of uncontrolled studies claimed success. Such a comparison is not even possible with my sample of the American literature since only 4, or 11 per cent., of their studies were controlled.

Table I shows the percentage of treatments claimed as successful in 20 controlled and 52 uncontrolled British and American studies.

TABLE I
72 Anglo-American Papers

	Success	Failure	Total
Controls	5	15	20
No controls	43	9	52
Total	48	24	72

$n=1$ $\chi^2=21.06$ $P<.001$

* Based on a paper read at the Annual General Meeting of the Royal Medico-Psychological Association in Oxford in July, 1957.

Thus 28* per cent. only of the studies used some sort of controls; in uncontrolled studies success was claimed in 83 per cent. as against 25 per cent. in the controlled studies (and in some of these appropriate statistical treatment was lacking). For one degree of freedom, χ^2 is 21.06 and $P < .001$. It would appear, therefore, from this study of the literature that one can safely conclude that claims for the success of a treatment are closely associated with absence of the means whereby these claims can be scientifically substantiated. It has been argued, in spite of the above evidence, that the author is flogging a dying, if not a dead, horse. The percentage of controlled studies in 1957 so far is little more than half that in 1956.

FLORENCE NIGHTINGALE AND THE FAIRY FROM OUT OF THE WOOD

In a B.B.C. pantomime skit a character kept appearing with the introduction "I am the fairy from out of the wood and I goes about a-doing of good". Needless to say her good endeavours invariably resulted in the creation of chaos. This, in more technical language, is sometimes referred to as the flight into activity.

When Florence Nightingale first went to the Crimea, she found men dying in the most appallingly filthy conditions. Her assistants wanted to start nursing at once. She very courageously stopped them on the grounds that their work would be piecemeal and hindered at every turn until they had obtained the full co-operation of the medical men on the spot and of the Government at home. A few more men died; but many thousands have since been saved.

A resolute determination not to put the humanitarian cart before the scientific horse has not been a striking characteristic of psychiatry. Some years ago one of the treatments in vogue was as follows: "A drink for a fiend-sick man, to be drunk out of a church-bell: Githrife, cynoglossum, yarrow, lupin, flower-de-luce, fennel, lichen, lovage. Work up to a drink with clear ale, sing seven masses over it, add garlic and holy water, and let the possessed sing the *Beati Immaculati*: then let him drink the dose out of a church-bell, and let the priest sing over him the *Domine Sancte Pater Omnipotens*." Doubtless control groups were then—as so often now—thought to be unethical, so that it would have been impossible to determine whether, in such cases as appeared to respond well, the therapeutic agent was the drink, the *Beati Immaculati* or the *Domine Sancte Pater Omnipotens*.

Obviously the clinician in charge of a case, with the great responsibility which this involves, must have the final decision on the question of giving or withholding treatment; but is the argument usually advanced against the withholding of treatment a valid one?

Insulin Therapy was introduced before its efficacy had been established by adequate experimental methods. A large number of clinical psychiatrists came to believe, on the basis of uncontrolled observations, that insulin was in fact efficacious in certain cases. This being so, they were undoubtedly right to refuse to withhold this treatment in such cases. Their integrity cannot be questioned; but the basis for their belief can and should be. During the past few years an increasing number of clinicians have begun to doubt this belief. To some of these faults of the ageing mistress suddenly became apparent with the advent of the beautiful young Serpasil. Soon a large number of clinicians will come to believe, on the basis of uncontrolled observations, that Serpasil . . . but perhaps this particular human characteristic has been adequately dealt with in Anatole France's *Penguin Island*, where—it may be remembered—the

beginning of the epilogue describes the demolition of all skyscrapers and the end their re-erection. Meantime controlled experiments are either lacking or, as in the case of the long-term follow-up study by Penrose (1944) and the controlled study by Lehoczky *et al.* (1939) showing negative results for Insulin, so scotomatized that the recent paper by Ackner *et al.* (1957) came as a surprise.

The same story can be told of the electrical and neurological procedures, with perhaps the added weakness that the groups for which the treatment was originally thought to be most efficacious have gradually changed and always towards those groups which are thought to have the best prospects of spontaneous recovery. The drift has been with E.C.T. and leucotomy away from schizophrenia towards depression. More recently with the pharmaceutical procedures, such as largactil, and the inspirational, such as carbon-dioxide, less ambitious workers have gone straight to the group with apparently the best chance of spontaneous recovery, the anxiety states; or, if need be, to the somewhat nebulous "tension states", who will soon be definable as those who respond to a given treatment.

When a new treatment is suggested no one knows whether or not it will work. Clearly it is at this stage that it should be assessed by adequate experimental methods before ethical questions are introduced. These ethical questions arise because clinicians believe the results of reports such as those discussed below. Such reports are, however, nothing more than the dissemination of folk-lore. The withholding of treatment X cannot be unethical if it is not known to be efficacious. Continuing to give treatments with no intention of validating them would seem to be the more immoral course.

LACK OF DESIGN IN MUCH PSYCHIATRIC RESEARCH

A combination of confused ethical and methodological judgments has resulted in a succession of publications which are treated as research publications, but which are in fact simply the chronicling of clinical routine. Much of the methodological confusion seems to be due to the dichotomizing of clinical observation and experimental method, which are thought of as antithetical approaches to the same type of data at the same time. No scientist is likely to deny the value of the subjective observations which frequently lead to the setting up of experiments. Unfortunately many investigators consider their task complete after these uncontrolled observations have been made. The Argyll Robertson phenomenon has been cited in support of the view that such observations can stand independently of statistics; but its practical importance for psychiatry is dependent upon its occurrence in roughly two out of every three cases of GPI and its extreme rarity in other conditions. This is a statistical concept—even though the sums may not have been done. It is not even always necessary to do them. We do not require tests of significance to satisfy ourselves that the sun sets more frequently in the west than in the east; but, until Hume has been convincingly refuted on other than epistemological grounds, this must remain a statistical concept. Who knows what tomorrow may bring?

Concern here will be with those investigations which purport to assess the value of a psychiatric treatment. The following report illustrates one of the commonest procedures.

An adequate number of psychoneurotics were given drug 1 and then assessed as remitted, improved and unimproved. About 60 per cent. were thought to have gained substantial benefit from the treatment. Of the subgroups, anxiety states did the best. The conclusion is drawn that the main effect of the drug is the relief of tension. This claim is, of course, utterly un-

substantiated. This type of conclusion can only be drawn from intra-group and not from inter-group designs. To justify such a conclusion many other variables would have to be controlled. The following well-known story may bring out the logical fallacy more clearly. A conjuror, performing at a ship's concert, pointed to a parrot in a cage and said he would make it disappear. At the crucial moment the ship struck a floating mine and blew up. All were thrown into the water, including the parrot which was to be seen, with all its feathers off, pacing up and down on a floating door muttering "bloody clever". In this story it was the parrot-patient rather than the conjuror-doctor who was deceived; but it does serve to point up the fact that variables as large and efficacious as mines may intervene unremarked.

Since—to return to the investigation—psychoneurotics were apparently taken at random, one would expect that they were being regarded as a group and that an intra-group design would be used; but the stated aim was to assess the effect of previous personality on the results. In such a case psychoneurotics, regardless of diagnosis, with pre-breakdown psychoneurotic traits of personality and those without such traits should be compared. Had the aim been to assess the value of drug 1 with psychoneurotics in general, matched groups on drug 1 and placebo 2 should have been compared. Or again, had the aim been to discover whether personality or the syndrome were the decisive factor, anxiety states and hysterics (the only groups of any size in the study) should have been divided into those with and without pre-illness neurotic traits and the design could have been: AX_1 ; AX_2 ; AY_1 ; AY_2 ; HX_1 ; HX_2 ; HY_1 ; HY_2 —where A is anxiety, H is hysteria, X having neurotic traits, Y not having them, 1 is the drug and 2 the placebo.

By comparing $(AX_2 + AY_2) - (AX_1 + AY_1)$, the effectiveness of the drug with anxiety states could be determined. By $[(AX_1 + AY_1) - (AX_2 + AY_2)] - [(HX_1 + HY_1) - (HX_2 + HY_2)]$ it could be determined whether favourable response to drug 1 was a characteristic of anxiety states which distinguished them from hysterics.

By $[(AX_1 + HX_1) - (AX_2 + HX_2)] - [(AY_1 + HY_1) - (AY_2 + HY_2)]$ it could be determined whether favourable response to drug 1 was a characteristic of patients with pre-illness neurotic personality traits which distinguished them from those who do not have such traits. Characteristics of these groups are all that can be determined by such inter-group comparison.

In a second study an adequate number of depressives received drug 2 and were then assessed as remitted, improved or unimproved. No evidence of clinical improvement attributable to drug 2 was found. One cannot, however, arrive at negative conclusions any more happily than at positive ones in the absence of intra-group controls. Without drug 2 these patients might have got worse.

A third study is concerned with treatment Z in mental disorders. The total number of subjects (25) was subdivided into 6 groups. Even so, a list, admittedly tentative, of indications and contraindications was given. Sixty per cent. were again improved. Three pages were devoted to theories of action—that is to how it works before it is known whether it works. Such discussions are irrelevant to inter-group designs and can claim no superiority over armchair discussions, rather the contrary since they mislead many people into thinking that they are based on facts. One of the principal arguments put forward in favour of treatment Z is that it works with patients who have failed to respond to anything else. It is admitted that only 60 per cent. of the cases were treated successfully and that this is no better than many other treatments. One would

imagine that by now, with all the treatments that are moderately successful with cases that have not responded to all other forms of moderately successful treatment there could scarcely be anyone left who is not at least much improved; yet the chronic wards seem to remain remarkably full.

COMPARISONS AMONG AND BETWEEN GROUPS

Most of the points in this section have been more fully and ably presented by Kline (1953), but some repetition is necessary for the main argument of this paper.

Intra-individual comparisons: This method applies when the procedure to be investigated is reversible within a short time, when the number of subjects is unavoidably small and when the variables are either largely unknown or numerous. The effect of sodium amytal on catatonic stupor may serve as an example. Let it be supposed that 12 stuporose patients have been collected and that the degree of stupor can be measured on a five-point scale. The procedure would be to divide the 12 into two groups as well matched as possible. Group A would be measured for stupor, then given sodium amytal, again measured for stupor, allowed time for the effects of the sodium amytal to wear off, measured for stupor, given a placebo, measured for stupor. Group B's procedure would be: measured, given placebo, measured, interval, measured, sodium amytal, measured.

If some such results as the following were obtained: A: 5—sodium amytal—1—interval—5—placebo—5 and B: 5—placebo—5—interval—5—sodium amytal—1, it might be concluded that sodium amytal tends to bring catatonics out of stupor.

If the following results were obtained: A: 5—sodium amytal—4—interval—3—placebo—2 and B: 5—placebo—5—interval—5—sodium amytal—4, it might be concluded that sodium amytal was not so reversible after all. Or again, something like this might be found—A: 5—sodium amytal—4—interval—3—placebo—2 and B: 5—placebo—4—interval—3—sodium amytal—2, in which case additional attention or suggestion might be accounting for the results.

Should the experiment require the comparison of three quickly reversible drugs and a placebo, 16 cases all of one diagnostic category might be collected and the following design adopted:

- A: 1—2—4—3
- B: 2—3—1—4
- C: 4—1—3—2
- D: 3—4—2—1

This would control the effects of position and sequence and the value of the treatments could be assessed by Analysis of Variance.*

Inter-group comparison: This method may be used to ascertain characteristics of groups which distinguish them from other groups. Suppose that the hypothesis to be tested is that, relative to anxiety states, depressives tend to go off to sleep more quickly, wake sooner and are then unable to get to sleep again; whilst the anxiety states, relative to depressives, are slow to get to sleep, but wake later and more bemused. Two groups are selected, matched on some at least of the variables likely to be relevant, and the necessary observations and recordings are made. If the hypothesis were supported by the observations, it could be concluded that such and such are the characteristic

* Since the presentation of this paper just such an experiment has been designed by Fraser Roberts and reported by Raymond *et al.* (1957).

sleep habits of depressives as contrasted with anxiety states and conversely. Nothing can be said about processes. It cannot, for example, be concluded that depression causes people to wake up in the middle of the night. It might be found subsequently that most people other than anxiety states tend to wake up in this way. If it could be established that this particular sleep rhythm occurred with greater frequency among those who were depressed than among those who were not, it would still be necessary to show that this sleep rhythm occurred only when these particular individuals were depressed and not when they were well.

Intra-group comparison: This method may be used for testing either reversible or irreversible procedures. If it be desired to assess the effect of brief stimulus therapy on anxiety states, criteria must be set up to enable the group to be made as homogeneous as possible. When as many variables as may be have been matched, randomization should be used in the hope that any unknown variables will break even between the groups. Pre-treatment measures are then taken for each group and one given BST, the other a dummy treatment. The pre- and post-treatment differences of the two groups can then be compared. Something of this sort has, in fact, been done by Montagu and Davies (1955).

There is no doubt that the difficulty of obtaining control groups is often very great in psychiatry, perhaps particularly so in irreversible procedures such as leucotomy. The answer is not, however, to do studies without them and then draw conclusions which could only be drawn had they been used. Once we have found in controlled experiments a treatment that works we can, of course, use that treatment as our yard-stick for evaluating subsequent treatments, and control groups in the sense of untreated patients would no longer be necessary. If the findings of Raymond *et al.* (supra) are confirmed, such a yard-stick for drug trials will be available in amylobarbitone. A second best method is available and has been used, for example, by Petrie (1952) when she compared two different surgical insults; but all this tells us is that one is a cut above another. It does not tell us whether the better is better than nothing.

IN CONCLUSION

A conference on Mental Health in Oxford, attended by eminent psychiatrists and interested scientists, discussed research for several days. The proceedings were reported in a book entitled *Prospects in Psychiatric Research* (1953). No mention was made of the need to train research workers in methodology. Without some such training the prospects seem to me to be disturbing. Particularly at this time when new treatments are being produced monthly together with attractive brochures, more attention must be paid to the design of experiments if the danger is to be avoided of slipping back into something akin to Mediaeval Alchemy—dose for a fiend-sick man by courtesy of Messrs. A and B. A Special Medical Correspondent in *The Times* defended the Medical Research Council's relative lack of support of research in Mental Health chiefly on the grounds that insufficient people capable of doing such work were available. Must we then sit back and wait for some Minervaesque research workers to spring fully armed from the head of Jove? If, when we think of research, we think of the cause of schizophrenia, perhaps we should. If, on the other hand, we think as well of such comparatively mundane tasks as the one discussed here—the evaluation of the work of our "drug addicts", or of the somatherapies recently so carefully reviewed by Staudt and Zubin (1957), then surely the answer is "no". Here, if anywhere, is a field in which

even the guiding hand of a genius is unnecessary. Here, if anywhere, the less gifted can do a useful job of work. It should be someone's responsibility to see that there are more of us.

REFERENCES

1. ACKNER, B., HARRIS, A., and OLDHAM, A. J., *Lancet*, 1957, *i*, 607-611. 23 March.
2. KLINE, N. S., *Psychiat. Quart.*, 1953, **27**, 3, 474-495.
3. LEHOCZKY, T., ESZENYI, M., HORANYI-HECHST, B., and BAK, R., *Z. ges. Neurol. Psychiat.*, 1939, **160**, 24.
4. MONTAGU, J. D., and DAVIES, L. S., *J. Ment. Sci.*, 1955, **101**, 424, 577-592.
5. PENROSE, L. S., *Ontario Dept. of Health*, 1944.
6. PETRIE, A., *Personality and the Frontal Lobes*, 1952. London.
7. RAYMOND, M. J., LUCAS, C. J., BEESLEY, M. L., O'CONNOR, B. A., and FRASER ROBERTS, J. A., *Brit. Med. J.*, 1957, 63-66. 13 July.
8. STAUDT, V. M., and ZUBIN, J., *Psychol. Bull.*, 1957, **54**, 3, 171-196.
9. TANNER, J. M. (Ed.), *Prospects in Psychiatric Research*, 1953. Oxford.

SUICIDE IN HONG KONG*

By

P. M. YAP, M.A., M.D.(Cantab.), D.P.M.(Lond.)

Psychiatric Consultant, Hong Kong Government
Lecturer in Psychiatry, Hong Kong University

I.—INTRODUCTION

Comparative Studies on Suicide

ENQUIRY into the occurrence and the pattern of S.† in different cultures promises to reveal information of importance, but there is as yet inadequate material from which definite conclusions can be drawn. Cavan (1928) and Dublin and Bunzel (1933) devoted chapters of their works on S. to this aspect of the subject. These, however, were but poorly assorted collections of descriptive items from various ethnologists, orientalist, and historians, and from them few significant relationships between S. and culture could have been deduced. Zilboorg (1936, 1937) tried to build a concept of S. as a preformed, archaic behavioural reaction on ethnological data, but it must be admitted that valuable though this formulation is he has made somewhat tendentious use of his material in weaving them into the fabric of psycho-analytic theory. His arguments have been subjected to criticism by Wile (1937). Ellenberger (1953) cited examples of S. from various cultures that could be classified under the three components of the S. impulse described by Menninger (1938), viz., the wish to die, the wish to be killed, and the wish to kill. Such a classification, however, cannot cover all the facts. The social sanctioning of S. for different motives in different cultures would seem to argue against the notion that S. everywhere involves the same "components". The view that S. is rare in elementary and compactly organized societies has long been held, and has recently been confirmed as far as the Yorubas of West Africa are concerned by Lambo (1956).

Studies of S. in the region of South-east Asia have been published by Anzures (1927) in the Philippines, by Gunasekara (1951) and Strauss and Strauss (1953) in Ceylon and by Murphy (1954) in Singapore.‡ These studies revealed that the S. rate varied among different races even though they lived within the confines of one country, but also that many of the social factors that have been shown to influence the rate in western countries operated in the same way in these cultures. Murphy's paper dealing as it does with a predominantly Chinese population provides important material for comparison with our findings. The report on mortality from S. published by the World Health Organization (1956) gives expression to the significant trend of interest in S. studies towards comparative statistics, but most of its data were drawn from western countries, and Ceylon and Japan were the only two countries representative of Asian cultures for which statistics were given.

Published information from China proper is meagre, at least as regards

* Abstracted from a thesis approved for the degree of M.D. by Cambridge University.

† The abbreviation S. will be used for Suicide, A.S. for Attempted (unconsummated) Suicide, and S.A. for Suicidal Acts, i.e. S. plus A.S.

‡ A study of the subject in Japanese by Dr. G. de Vos of Ann Arbor, Michigan, is in the press.

literature in English. A systematic social survey of Peking by Gamble and Burgess (1921) gave certain S. data for that city to which we shall later refer, but a rate for the whole of China is not available. Crude data reported by different hospitals have been published, and they are of limited value in the absence of basic population figures (see, e.g. *China Medical Journal*, 1918, 32, 371; 1923, 37, 420). The subject of S. in Chinese women, which as we shall see is of special interest because of their traditional cultural background, was briefly annotated in the same journal (1924, 38, 503). Raw data on S. classified according to age, sex and ethnic group are also to be found in the reports of the past Shanghai Municipal Council. I am not aware of any other material in English on this subject in China, although it may be pointed out that Chen Ta (1946, pp. 100, 101) in carrying out his census studies in South-west China obtained rates for "suicide and poisoning" for the Kunming district. The present study is the first full-scale investigation of S. in a Chinese population, albeit one that is politically and economically (but not geographically and socially) distinct from that of China.

The comparative study of S. is of value because it brings into relief the question of the "normality" of this behavioural reaction. It helps to rescue discussion of this subject from the dangers of over-simplification, and enables us to define clearly the basic problem that it presents for psychiatry. For this purpose it is necessary to obtain and analyse data from cultures as distinct as possible from western civilization, and view the subject from a broad "anthropological" standpoint, as Strauss (1956) has usefully attempted to do in spite of the limited information at present available. Only from studies of this kind can we hope to understand the S. reaction as a unique aspect of human behaviour irrespective of cultural conditioning, e.g. its fundamental relation to maleness and old age.

The present work attempts firstly to analyse certain statistical aspects of S. in Hong Kong and relate them to social conditions and the cultural background. Secondly, it examines in greater detail the findings concerning A.S. in order to come to some understanding of the latter from the psychopathological as well as the clinical point of view. The period chosen for study was determined by the existence of base demographic and socio-economic data derived from the Hambro Sample Survey of 1954.

II.—SOURCES AND METHOD OF INVESTIGATION

The data for this study were obtained from the following sources:

(a) The Registry of Births and Deaths had records of all deaths from S. in the Colony of Hong Kong (comprising Hong Kong Island, Kowloon and the New Territories) for the post-war years. Earlier records had unfortunately been destroyed. In comparing figures from this source with those from the Criminal Investigation Department, it was found that the latter had somewhat lower totals for the annual number of S., and this discrepancy appeared to raise some doubt as to the accuracy of the Registry data. However, the Registry figures included cases of presumed S. labelled as such by the Coroner after full enquiry, including *post mortem* examination, whereas such cases might be reported by the police only as "found dead".

To incriminate S. as the cause of death when a body is found in the sea, for example, is not entirely a matter of guess-work. Reliable indications can be obtained from the age of the deceased, evidence of his occupation (seamen and fishermen could be excluded from suspicion), examination of stomach contents, signs of serious chronic illness, the existence of S. notes, as well as

customs (albeit rarely observed) like putting on one's best clothing before facing death, or wearing red trousers and keeping on oneself a pair of scissors in the case of vengeful S. The number of deaths from S. recorded for the calendar year 1955 was 263, and included 43 "presumed" cases. The compactness of the whole Colony, which covers only 391 square miles, as well as the heavy concentration of population in urban areas, make it probable that both the registration of deaths from S. as well as the reporting of A.S. cases to the police were quite complete, except perhaps that one could never expect the A.S. figures to reflect the true incidence as accurately as the S. For the first or second year after the war, however, the figures may not be quite reliable because it took time for the administration to be fully re-established.

(b) The Criminal Investigation Department kept records of all S. and A.S. cases, although data gathered on the latter were frequently incomplete. The dossiers of S. cases were compiled by experienced Inspectors of Police from information obtained usually from several persons, and were forwarded to the Coroners. Their efforts were directed towards finding the cause of death and the exclusion of foul play. There was evidence that the question of insanity was regularly brought up during interviews with those who had known the deceased, as well as the possibility of other, social and economic, precipitating causes. I was able to go through 218 dossiers covering the period November, 1953 to December, 1954. These provided reasonably detailed information on the social background of the cases. From other records kept by the police it was possible to obtain data on the age and sex of 1,158 cases of S.A. (not counting repeated attempts) and the mode employed in 1,418 cases of S.A. within the period June, 1953 to December, 1954. The last figure is the total of all the cases occurring during that period, again excluding repeated attempts. As regards the latter, second attempts were made by 2 men and 4 women, but in 2 of the women the age was not known; none of these attempts were consummated.

Of the 1,418 cases of S.A., all were Chinese except for 1 S. case in a British soldier and 2 A.S. cases in a British merchant and a Portuguese woman (unemployed). The sample may therefore be regarded as one drawn from the Chinese population altogether. However, these 3 cases were excluded in analysing the effect of the factors of immigration, nativity, civil status, occupation and precipitating causes.

(c) In order to obtain sociological data on cases of A.S., it was necessary to turn to the records of the Almoner's Department in Queen Mary Hospital, the largest in the Colony, serving the whole of Hong Kong Island. All cases of S.A. on the island are brought by the police to this hospital in the first instance, and here they are interviewed by almoners before referral, if indicated, to the Social Welfare Department for assistance. If insanity is detected by the medical staff the case will be referred to the author as Government Psychiatric Specialist, or directly admitted to the Mental Hospital. These insane cases were included in the C.I.D. records mentioned above in (b), although they were of course drawn only from Hong Kong Island, but as insane cases they may be regarded as representative of all such in the Colony of Hong Kong.

Unfortunately the almoners' histories were not complete in many cases. Excluding those of persons who later died, I was able to obtain data on occupation in 127 cases, and on civil status and precipitating causes in 159 cases—all occurring between June, 1953 and December, 1954. There is no reason to suppose that, when information was wanting, this was directly related to any selective influence arising from just those sociological characteristics for which

we were seeking. The completeness of the histories depended primarily on the seriousness with which the casualty officer viewed the case, and also to a certain extent on the interest of the almoners. which varied considerably.

Control Data

(a) Estimates of the annual population of Hong Kong for the post-war years were obtained from Szczepanik (1955), who had subjected the overall population data of the Hambro Report (1955) to criticism. Szczepanik was able to come to a more accurate estimate of post-war population changes in Hong Kong by applying the demographic principle that the age-structure of a population tends to remain constant, despite temporary changes introduced by immigration waves.

(b) The Sample Survey of Hong Kong's population carried out in May-June, 1954 by Hambro (1955) contained information on 17,682 persons derived from 4,600 questionnaires, and involved a sampling fraction of 7.86 persons per 1,000. In the absence of a recent census (the one planned for 1951 had to be given up because of the outbreak of the Korean War), this Survey provides the most accurate analysis of the social and economic structure of Hong Kong's population, but as regards age and sex structure it unfortunately fails to give sufficiently detailed information for our purpose. It should be noted that 99 per cent. of Hong Kong's population are Chinese.

(c) In order to obtain a more detailed analysis of the age and sex structure of the population, the data from the Hambro Sample Survey were supplemented by those from the Medical Department Survey based on identity cards in 1950.

(d) A small survey to ascertain the distribution of civil status in the population was carried out. This covered 1,136 persons aged 15 and over.

III.—DISCUSSION OF THE FINDINGS

A. THE SUICIDE RATE PER ANNUM

From Table I it can be seen that the crude rate per 100,000 of the total population *per annum* was quite low in the immediate post-war years, but rose gradually, took an abrupt step upwards in 1952, and remained at a high level

TABLE I
Annual Suicide Rates in Hong Kong Per 100,000 Total Population

Year	No. of Cases			Mid-year Population	Rate		
	M.	F.	Total		M.	F.	Total
1946	16	19	35	1,168,000	—	—	3.0
1947	35	26	61	1,214,000	—	—	5.0
1948	31	34	65	1,310,000	—	—	5.0
1949	60	51	111	1,415,000	—	—	7.8
1950	88	56	144	1,670,000	—	—	8.6
1951	90	75	165	1,846,000	—	—	8.9
1952	149	107	256	1,967,000	—	—	13.0
1953	167	99	266	2,050,000	14.8	10.0	12.9
1954	183	119	302	2,120,000	16.3	12.0	14.2
1955	167	96	263	2,185,000	14.4	9.3	12.0

Source: Registrar of Births and Deaths. Population figures from Szczepanik. Male:Female Population Ratio from Hambro Sample Survey.

after that. The crude rate of 12.0 for 1955 may be compared with those found for other countries from periodical data given in the *Demographic Year-book* published by the Statistical Office of the United Nations, and also from the special survey of the World Health Organization on mortality from S. (1956, pp. 250 ff.). It is a little higher than that for England and Wales (11.4 in 1954), the United States (10.1 in 1953) and Australia (10.9 in 1953). It is lower than that for France (15.8 in 1954) and Sweden (18.6 in 1953). In general the rate in recent years might be regarded as neither very high, like that for Germany (19.3 in 1954), Switzerland (22.6 in 1954) and Japan (23.4 in 1954), or very low, like that for Eire (2.0 in 1954) and Spain (5.9 in 1954).

Gamble and Burgess (1921) found that the crude rate for the total population of Peking City in 1917 was 15.5, but the rate for China as a whole may be expected to be lower. In Singapore, Murphy (1954) found a crude rate *per annum* of 12.9 for the total Chinese population for the years 1946–52. This compares well with the Hong Kong rate, so that our figure may perhaps be regarded as typical for Chinese living in urban communities in South-east Asia. The crude rate for Hawaiian Chinese was 9.0 *per annum*, taking the annual average for the years 1941–50, but the rate for the previous decade was more than double this figure, according to Taff (1951). Cavan (1928, p. 36) stated that the rate for Chinese in Hawaii between 1918 and 1922 was 34.0, for all ages,* and also that it was as high as 65.0 for Chinese in Chicago, taking the average of the years 1919–21 (*op. cit.*, p. 80). While the reliability of the rates for a comparatively small population like the Chicago Chinese may be questioned, this could not be the case with the Hawaiian Chinese figures, which have been showing a gradual decrease. Nevertheless, taking all Chinese in the United States, the crude rate for them is still very high, being 30.4 *per annum* for 1949–51 (see footnote below). If the trend in the Hawaii figures may be taken as an indication, the U.S. Chinese rate may fall with further assimilation into the country.

If the population aged 15 and over only is considered, the Hong Kong rate for 1954 was 23.5. This rate can again be compared with that found by Murphy for Singapore, which was 21.0 *per annum* for the Chinese population aged 15 and above, for a number of years between 1930 and 1952.

The upward trend of the crude S. rate in Hong Kong over the years and the abrupt rise in 1952 demand explanation. There were no basic administrative changes in the Registry of Births and Deaths, nor changes in the procedure for recording S. cases, in 1952 or any of the other years, so that the increase in the rate cannot be merely an apparent one. On the other hand, if we inquired into the post-war population changes and especially the inflow of refugees caused by the Communist revolution, with its adverse psychological repercussions, we should be surprised if the S. rate did not show the changes it did. The post-war growth of population of Hong Kong came about mainly from two waves of immigration: between 1945 and 1948 about 700,000 ex-Hong Kong citizens came back and then between 1949 and 1951 some 500,000 others, mostly refugees from the revolution who had not been previously in Hong Kong, came in (Hambro, 1955, pp. 17, 18; Szczepanik, 1955). These immigrant movements, balanced by the smaller amount of emigration going on during these years, resulted in the increase as shown in Szczepanik's figures (see Table I), and it may be noted that the increase was greatest between 1948 and 1952, reaching a peak in 1950/1951. The contribution from natural increase

* Cavan mentions that this rate was "double the Pekin rate for 1927" but gives no bibliographical reference for the latter figure.

was of course far less important a factor, compared with the balance of immigration, in bringing about the post-war increase of population.

The sudden rise in the S. rate in 1952 and the high rates in the following years were most probably due, not only to the simple fact of increase in the number of immigrants, but also to other factors like their age distribution as a group, difficulties in adapting themselves to Hong Kong, and political events in the neighbouring province of Kwangtung, from which most of the immigrants came. This problem will be further discussed under the section on nativity and immigration.

It has long been known that in war-time and during periods of political crisis the S. rate drops (cf. Halbwachs, 1930), and that this effect extends to adjacent non-belligerent countries (Faris, 1948, p. 213). The end of World War II in China brought about increasing tension between the Communists and the Kuomintang, but it was not until 1949 that the tide of revolution reached right down to the borders of Hong Kong. This, combined with the fact that the ex-Hong Kong persons pouring back into the Colony during the immediate post-war years might be expected to have been buoyed up by hope and optimism and therefore to be relatively free from S. risk, could account for the very low rates during those early years. The subsequent rise might be related, apart from the factors of immigration and political disturbance, to increasing post-war industrialization and perhaps to the decline of the entrepôt trade and consequent economic depression, following upon the application of embargoes against China. It has long been known that industrialization and economic depression increase the S. rate (cf. Dublin and Bunzel, 1933, p. 101; Faris, 1948, p. 207). The importance of these factors, however, cannot be adequately discussed without a knowledge of economics.

B. URBAN AND RURAL RATES

Table II sets out the S. and A.S. rates for two urban areas—Hong Kong Island, where almost the whole population is concentrated in the city of Victoria, and Kowloon, the twin city of Victoria on the mainland: and for one rural area

TABLE II

Suicide and Attempted Suicide by Locality: Cases Reported to Police, June, 1953 to December, 1954, Excluding Marine Cases

Locality	Suicides			Attempted Suicides		
	Total No.	M:F	Rate*	Total No.	M:F	Rate*
H.K. Island (Urban) ..	133	1.7	14.8	397	0.7	44.1
Kowloon†	171	1.4	16.3	463	0.6	44.1
(Urban)			15.6±0.88			44.1±1.48
N.T. (Rural)	26	1.4	8.7±1.60	79	0.7	26.3±0.39

* Per 100,000 total population.

† Includes the part of the city in the area of New Kowloon.

Source: C.I.D. records. Population data from Hambro Sample Survey.

—the New Territories, lying beyond Kowloon and extending to the border. The cases were assigned to the different areas according to the police stations investigating them. Each station was responsible for cases occurring within its own defined district, and the latter could be classed either as urban or rural. It was necessary to exclude 149 cases investigated by the marine police from

the total, because of uncertainty over their origin, although from their appearance most of the bodies found dead on the sea were those of city-dwellers, and also most of the unconsummated marine cases were persons picked up in the harbour itself between Victoria and Kowloon. The base population distribution between Hong Kong Island (900,000) Kowloon (1,050,000) and the New Territories (300,000) as given by Hambro in his Table XI included all those living on boats; and these in 1954 made up 7.8 per cent. of the total population (Hambro, Table III). But only 0.3 per cent. of total population were true fisherfolk going out to sea (Hambro, Table XXXI), and the difference between these two figures of 7.5 per cent. is made up of boat-dwellers who are in fact closely attached to the land, either rural or urban as the case may be. The percentage of true fisherfolk in the population is very small and therefore need not be considered in studying urban-rural differences, especially as they are distributed between Hong Kong, Kowloon and the New Territories.

Table II shows that in Hong Kong as elsewhere (cf. Cavan, 1928, p. 77 ff., and Dublin and Bunzel, 1933, p. 78) the urban S. rate is significantly higher than the rural, and that this also holds for A.S. Regarding S., the difference between the two rates is 6.9 ± 1.83 . For A.S., the difference between the rates is 17.8 ± 1.87 . An exception to the general rule that S. preponderates in cities was found by Schroeder and Beegle (1953) in Michigan, but their data showed that many in their apparently rural population were urban-orientated, possessing city values and ideas which in effect lessened the contrast between town and country. In our case it must be admitted that the New Territories population is not entirely homogeneous, there being, for example, in the semi-rural townlet of Tsun Wan many factory workers. But Tsun Wan's population of 51,000 (data for 1955 from the Medical Department) does not amount to more than one-sixth of the total New Territories population, which may therefore be regarded as rural on the whole.

The low rural rate is reflected in the low rates to be found for occupations like farming and fishing (see Table II). In fact only one farmer in our material committed S., while not a single fisherman did so. There are grounds for believing that it is not merely occupational distribution that brings about the urban-rural difference, but more fundamental factors like the impersonality and social disorganization of urban areas, where there are few meaningful relationships between individuals and much social isolation. This has been established for western countries by a number of workers (Cavan, 1928, pp. 77-105; Schmid, 1928, 1933, 1937, pp. 370-380; Mowrer, 1942, pp. 347-350; Faris, 1948, pp. 207, 8; and Sainsbury, 1955, pp. 75-8). Social conditions in Hong Kong, however, have been rather abnormal and it would be imprudent to ignore the great influx of refugees, and their concentration in the city to struggle for a new social and economic adjustment, as important factors directly influencing the S. rate. This may all be part of social disorganization, but it is necessary to note the existence of serious poverty as a causal factor, much more widespread as it is than in established cities of the industrial western countries.

Another point to note is that the sex ratios for S. as well as A.S. show no urban-rural difference. This suggests that urbanization has no differential effect ultimately on the S. risk for each sex.

C. AGE AND SEX SPECIFIC RATES

The age and sex specific rates for S. and A.S. are set out in Table III and also graphically represented in Figures 1 and 2. The factors of age and sex

TABLE III

Suicide and Attempted Suicide in Hong Kong by Age and Sex, June, 1953 to December, 1954*

Age	Suicides				Attempted Suicides			
	No.		Rate/100,000		No.		Rate/100,000	
	M.	F.	M.	F.	M.	F.	M.	F.
11-15†	—	1	—	1.4	2	3	2.4	4.2
16-20	2	8	1.8	8.7	15	48	13.2	52.2
21-25	21	13	19.6	14.9	88	149	82.2	170.9
26-30	24	19	23.6	21.9	76	144	74.6	166.5
31-35	23	19	27.2	26.3	91	69	107.5	95.5
36-40	19	14	22.1	17.7	39	43	45.3	54.4
41-45	16	12	28.5	22.3	33	23	58.7	42.7
46-50	5	8	10.8	16.5	15	10	32.2	20.7
51-55	15	8	55.6	23.9	8	7	29.7	20.9
56-60	4	2	21.3	8.2	12	3	64.1	12.2
61-65	6	5	53.4	44.8	4	2	35.6	17.9
66-70	5	4	74.2	41.9	2	4	29.7	41.9
71-75	3	2	100.1	73.4	1	2	33.3	73.4
76+	2	3	133.5	220.3	—	1	—	73.4
Total	145	118	571.7	541.6	386	508		

* One case of successful suicide in a girl of 6 and 260 cases of unknown age excluded.
† The number of persons in the population on which rates were based in this age group was actually estimated between the ages of 12 and 15 in our reference data. All the cases of suicide and attempted suicide in this group were between 12 and 15.
Source: C.I.D. records. Population data derived from the Medical Department Survey of 1950 and the Hambro Sample Survey.

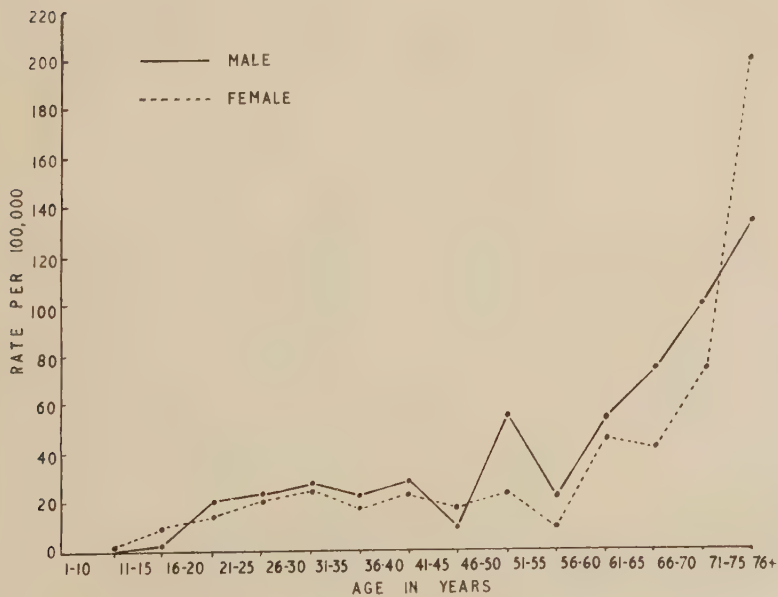


FIG. 1.—Suicide in Hong Kong, by age and sex.
(263 cases, June, 1953–December, 1954.)

Source: C.I.D. Records.

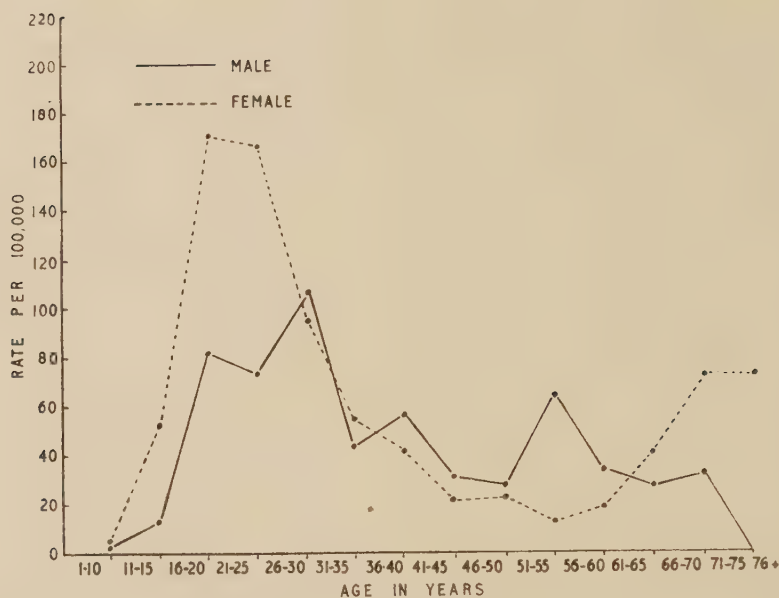


FIG. 2.—Attempted suicide in Hong Kong, by age and sex.
(894 cases, June, 1953–December, 1954.)

Source: Almoners' Histories.

furnish the most conspicuous bases for variation in the rates for S.A. and it will be seen that the curves for the two sexes are quite different. The trends revealed by these curves are definite, although it must be admitted that in old age the data may not be quite reliable, because old folk often cannot know their correct age and because the base population for these ages is small.

Suicide

As regards S., the male rate is rather lower than the female before the early twenties and the youngest male case is 5 years older than the youngest female. Then the male curve rises rapidly until the early thirties, when a plateau is reached, all the while keeping slightly above the female; and finally it takes a rapid climb from the early fifties, reaching well above the female, which latter also rises in the late fifties. These curves are similar to those found for most western countries, except that the plateaux between 30 and 50 are unusual and that the female curve rises sharply after the menopause, in Hong Kong, instead of reaching a peak at this period and then falling or remaining more or less stationary. From data given in the W.H.O. survey of mortality from S. (1956, pp. 275 ff.) it can be seen that with the definite exception only of Germany, France, the Netherlands and Portugal, every western country in recent times showed a stationary or falling rate in old age for women; this, however, was not true for the Asian countries Ceylon and Japan. Comparison may also be made with the data given by Cavan (1928, p. 314), Bond (1931), Dublin and Bunzel (1933, p. 382), Mowrer (1942, p. 339), Schroeder and Beegle (1953), and Schmid and van Arsdol (1955).

Our data show certain differences from those for Chinese in the U.S.* In the latter the male curve rises gradually to reach a peak in the sixties (and then apparently falls), but the female curve falls after reaching a peak at the menopause. This kind of pattern is much like that to be found in western populations.

If we turn however to the figures for Peking City in 1917,† calculated from the data given by Gamble and Burgess (1921), we find a remarkable difference. The male curve is highest for the age period of 31–40; after that it actually falls, and the tendency to rise in old age is not marked. The female curve is highest for the age period of 21–30 (it can be assumed that the rate could not be so high for the ages 11–20), and then it rapidly falls with increasing age. These facts can be well understood in the light of the traditional, conservative and patriarchal culture, with its great emphasis on filial piety and respect for the aged; with such an *ethos*, suicide-producing stresses must fall with especial severity on younger people, particularly young women constrained by the rigid family organization.

Thus it appears that with the breakdown of the traditional cultural pattern and the “modernization” of Chinese society S. has increased in old age for both sexes, and has decreased in young women. The curves then may be expected to approximate to Hong Kong’s, or to those found for Singapore (whose population is 80 per cent. Chinese) by Murphy (1954). The Singapore S. curves for males and females are similar to Hong Kong’s, although the rise in females with increasing age is not marked. Since, however, the curve for females does not rise in old age for American Chinese, it may be possible that high female S. rates in old age are peculiar to an Asian society in the transitional stages of “modernization”, and that when the rapid social, economic and cultural changes become stabilized the rates will fall. We have seen that in Ceylon and Japan too S. rates are high in older women. It may be objected that the rates for the older age groups are not reliable because of the small number of persons

* With the help of Mr. I. M. Moriyama (National Office of Vital Statistics, Washington), I have been able to calculate the following rates:

Age	Population (1950)		Mean Annual No. of S. 1949–51 inclusive		Rate/100,000	
	(M)	(F)	(M)	(F)	(M)	(F)
15–24	11,331	9,408	0·3	0·7	2·6	7·4
25–34	13,653	8,268	2·7	1·3	19·8	15·3
35–44	12,687	4,893	5·3	1·3	42·6	27·2
45–54	12,248	2,968	6·3	1·3	51·7	45·2
55–64	7,517	1,490	8·3	0·3	110·9	22·4
65–74	3,441	611	5·7	—	164·7	—
75–	1,145	181	1·7	—	116·4	—
All ages	76,725	40,415	30·7	5·0	40·1	12·4
					(M+F 30·4)	

(Source: U.S. Census of Population: 1950. Vol. IV, Special Reports, Part 3, Chap. B. Washington, 1953.)

† The data are as follows:

Age	Population (1917)		No. of S. (1917)		Rate/100,000	
	(M)	(F)	(M)	(F)	(M)	(F)
21–30	119,535	55,317	19	29	15·9	52·4
31–40	112,190	60,495	52	19	46·4	31·4
41–50	81,064	42,153	20	6	24·7	14·2
51–60	43,064	27,118	12	4	27·8	14·8
61–	30,572	23,774	12	3	39·2	12·6
21 and over ..	386,425	208,857	115	61	29·8	29·2

(Source: Gamble and Burgess, 1921, p. 416.)

involved, but the trend of the curves appears definite, and they show a sex difference.

Taking all ages into account, our male S. rate is higher than the female, but the latter is more than half that of the former. This is shown in Table I for the years 1953-55, the base male and female populations employed for the calculation being derived from the sex ratio of 113.7 M to 100 F found by Hambro in his 1954 sample survey (Hambro, 1955, Table XXV). This proportionately high female rate in relation to the male is not found in western countries, but is the rule in Asian countries like Japan (1.5:1 in 1954) and Ceylon (2:1 in 1954); in the west the ratio is usually 3:1 rather than 1.5:1 as in our case (cf. Dublin and Bunzel, 1933, p. 45; Mowrer, 1942, p. 339; Dahlgren, 1945, p. 43; Schroeder and Beegle, 1953; Strauss and Strauss, 1953; Schmid and van Arsdol, 1955, and the W.H.O. survey, 1956, p. 251). The Peking figures for 1917 were even more striking inasmuch as the female rate approximated to the male. Murphy found a ratio of 14.8 M to 10.8 F for the total Singapore Chinese population, which confirms our finding for Hong Kong. We have, however, seen that the male rate is higher rather than the female in American Chinese (40.1 M to 12.4 F.)

It appears therefore that there is in the present day no special "Chinese" pattern of S. as far as age and sex are concerned. Chinese living in a western environment show a pattern like that of the west as regards the sex ratio and also, as we have seen above, the declining female rate in old age, but not Chinese living in a "marginal" environment like that of Hong Kong and Singapore. In traditional China, however, there were as many females as males committing S., mainly because of the high rate in young women; the male rate, unlike that for Hong Kong or the west, decreased with age, relatively speaking, and the female rate, unlike that for Hong Kong but like that for the west, also decreased with advancing years. The influence of the cultural background on S. in Hong Kong will be more fully discussed later.

Attempted Suicide

We may now turn from S. to a consideration of A.S. The female A.S. rate is rather higher than the male (the rates per 100,000 total population being respectively 86.1 and 54.5). All workers agree that more females attempt suicide unsuccessfully than males and if we compare this ratio with those to be obtained from the data given by Lendrum for Detroit (Lendrum, 1933) and by Dahlgren for Malmoe (*op. cit.*, 1945) we find that it is higher than the former (1.3:1) and lower than the latter (2.0:1). However it is always difficult to be certain of the validity of data on A.S. because the adequacy of registration must vary at different times and in different places. The question of sex difference in S.A. will be further pursued below.

Returning to Figure 2, we see that for A.S. both the male and female curves rise steeply from the age period of 11-15, and after reaching their peaks, the female around 25 and the male 5 years later, they fall sharply until they cross each other in the early forties. Between 45 and 65 the male curve is higher than the female, but in old age the female curve alone rises to exceed the male again. On the whole these curves are similar to those found for western countries. Most studies in A.S. give only the absolute figures, but Lendrum (1933), Piker (1938), Dahlgren (1945, p. 51), and Schmid and van Arsdol (1955) give the rates for different ages. Lendrum's data are plotted out by Mowrer (1942, p. 343). In general the A.S. rate does not rise in old age for either sex, although

Piker found a rather high rate for men over 60; but his material of 1,383 cases included 224 known to have died later. In our findings the rise in the female curve after 65 is unusual. The rate for the older ages are based only on a small number of cases, and to this extent we must be cautious in assessing their significance; but the rise parallels that for S. and the trend is definite. It is perhaps possible to find an explanation for this in the restricted position accorded to women in the traditional culture.

Further Analysis of Age and Sex Distribution in Suicidal Acts

The data on S.A. can be made to yield further information if submitted to statistical analysis. Table IV shows that for S. the observed distribution

TABLE IV
Observed and Expected Distribution of Different Age Groups of Each Sex in Suicide and Attempted Suicide

Age	Suicide				Attempted Suicide			
	M.		F.		M.		F.	
	Obs.	Exp.	Obs.	Exp.	Obs.	Exp.	Obs.	Exp.
11-35 ..	70	95.27	60	71.03	272	250.60	413	305.81
36-55 ..	55	41.76	42	37.17	95	111.16	83	160.02
56- ..	20	7.97	16	9.79	19	21.23	12	42.16
Total	145	145.00	118	118.00	386	386.00	508	508.00
$\chi^2 =$	29.04		6.26		3.90		96.21	
P =	<0.001		0.02 < P < 0.05		0.1 < P < 0.2		<0.001	
			Df=2					

differs significantly from the expected in different age groups; both for males and females the number of S. cases is lower between 11 and 35, and higher after 35 (and especially after 55) than would be expected from the size of the age groups in the population if the S. tendency was constant for all ages. As for A.S., the difference in the distribution between observed and expected numbers for different age groups is significant only in the case of females; cases are more numerous than would be expected between 11 and 35, and less numerous after 35 and especially after 55.

We may next consider the question of sex distribution in S. as compared with A.S. First of all, the difference between the sex distribution of S. and that of A.S. is statistically significant: 145, or 55.2 ± 3.07 per cent. of 263 S. cases were men, and 386, or 43.3 ± 1.66 per cent. of the 894 A.S. cases were men. The difference between the two is 11.9 ± 3.49 per cent., and is highly significant. The preponderance of men in cases of suicide which succeed implies that S.A. of men are more likely to be fatal, and this is found by all workers to be so. Why women should be less successful in S.A. will be taken up later.

Although there is a significant difference in sex distribution between S. and A.S., we need also to know if the sex ratios in S. as well as in A.S. are themselves significantly different from the sex ratio of the general population. For this purpose it is not sufficient simply to compare the total numbers of men and women in the population, because, as we have seen, the S. tendency in both sexes (and the A.S. tendency in females) differ for various age groups, and the younger age groups are larger than the older in each sex.

If correction for differing strength of the S. tendency in different ages is made, the expected total numbers for S. in males and females, on the assumption that there is no sex difference in the S. tendency, are 140.4 and 122.6

respectively. Table V shows that in fact the proportion of sexes among the S. cases does not differ significantly from the proportion of the sexes in the general population.

TABLE V
Observed and Expected Distribution of the Sexes in Suicide

Age	Observed			Expected		
	M.	F.	Total	M.	F.	Total
11-35 ..	70	60	130	73.55	56.45	130.00
36-55 ..	55	42	97	50.85	46.15	97.00
56- ..	20	16	36	16.03	19.97	36.00
Total ..	145	118	263	140.43	122.57	263.00
$\chi^2=0.32$			Df=1	0.5 < P < 0.7		

In the case of A.S., Table VI shows that the total expected numbers, on the assumption that there is no sex difference in the tendency to A.S., are 494.9 for males and 399.2 for females, when correction is made for the age difference in the liability to A.S. Thus the proportion of the sexes among the A.S. cases differs significantly from the proportion of sexes in the population; fewer males and more females attempt S. unsuccessfully than would be expected from their respective numbers in the population.

TABLE VI
Observed and Expected Distribution of the Sexes in Attempted Suicide

Age	Observed			Expected		
	M.	F.	Total	M.	F.	Total
11-35 ..	272	413	685	387.70	290.30	685.00
36-55 ..	95	83	178	93.35	84.65	178.00
56- ..	19	12	31	13.80	17.20	31.00
Total ..	386	508	894	494.85	399.15	894.00
$\chi^2=53.69$			Df=1	P < 0.001		

Ratio of Suicide to Attempted Suicide

An important aspect of the study of S. that is often neglected is the total incidence of S. in relation to that of A.S. within a given population. The difficulty here is that the registration of A.S. cannot attain as a rule the degree of completeness that is possible with S., and that usually A.S. statistics are derived from hospital admissions, whereas S. statistics come from the Police or other Governmental sources. As far as Hong Kong is concerned, however, there is no serious stigma attached to S. among the Chinese, so that concealment of S.A. cannot be an important factor determining the reliability of our data. Moreover, A.S. is more easily concealed than S., so that if it is the case that the A.S.: S. ratio is high, then this fact may be of some significance.

A glance at Figure 3 will show that in both sexes the Hong Kong ratio of A.S.: S. is rather high in ages between 20 and 35, especially among females. This means that the mortality for S.A. in these groups is low. The question arises whether this finding is peculiar to Hong Kong, inasmuch as it must have some bearing on the psychology of the S. act, and may possibly be influenced by the cultural background.

Comparison of the A.S.: S. ratios (i.e., total number of A.S.: total number of S.) from different countries is of limited value, quite apart from the difficulties above-mentioned, because the age and sex structure of different populations varies and it is known that age and sex are factors influencing the mortality from S.A. However, comparison may usefully be made if it is between the same age groups and the same sex. Figure 3 shows further that there are

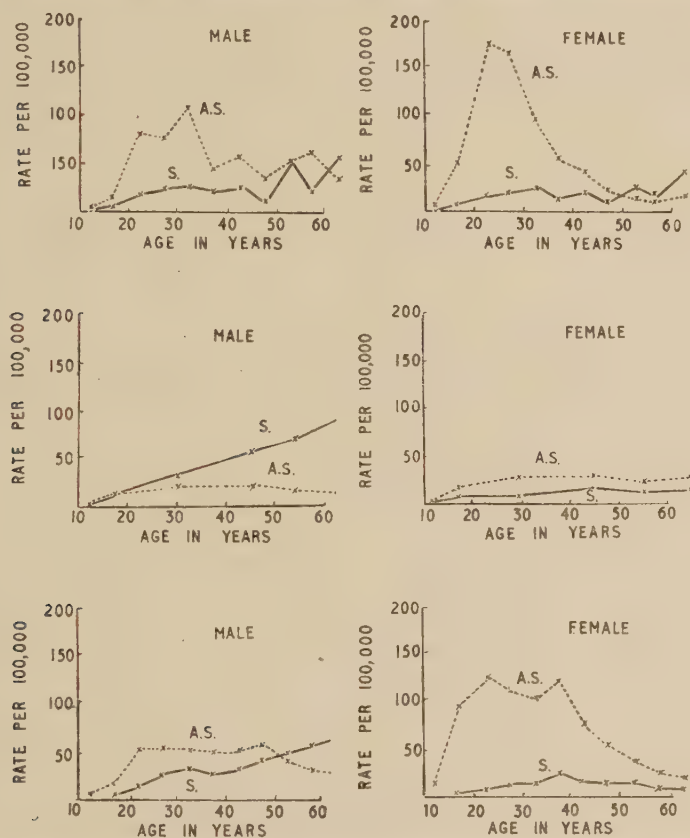


FIG. 3.—Suicides and attempted suicides in Hong Kong, Malmoe and Seattle, by age and sex. Sources: Almoners' histories and C.I.D. records; Dahlgren (1945); and Schmid and van Arsdol (1955).

definite differences between the A.S. and S. curves for the two sexes in Hong Kong, Malmoe and Seattle, when plotted out for various ages. Malmoe and Seattle are modern cities not comparable with Hong Kong, which is very much of a city-state in itself; and they are two of the very few cities for which adequate A.S. and S. data covering fairly recent years are available (Dahlgren, 1945; Schmid and van Arsdol, 1955). It is clear that in Hong Kong male and especially female S.A. have a lower mortality than in Seattle, and the mortality is even lower still when compared with that for Malmoe. We cannot conclude that this low mortality is peculiar to Hong Kong, but at least it can be argued that Hong Kong is one of the countries where S.A. in the young are quite frequently unsuccessful or unconsummated.* The relation of this to the social and cultural background is further discussed below.

* In Hawaiian Chinese the crude A.S.: S. ratio is smaller than it is for Hawaiian Caucasians, according to data for 1936-50 (Hsu, 1953, p. 63). It is difficult to draw any definite conclusions from this.

D. NATIVITY AND IMMIGRATION

In discussing the S. rate *per annum* for the post-war years I mentioned the importance of the factor of immigration in bringing about the rise in the rate after 1952. Before we deal fully with this problem we may first study the incidence of S. in relation to the "native places" of the subjects. The "native place" is the ancestral village of the subject's family, and in the great majority coincides with his normal place of residence, i.e., in the case of immigrants, the place from which he comes. It was possible to obtain data on the "native place" for 137 cases, taken consecutively in going through the dossiers. Table VII shows that those whose "native places" were in Hong Kong itself, or in

TABLE VII
Distribution of Suicide by Nativity

Native Place	Percentage Distribution in Population*	Suicide	
		Observed	Expected
Hong Kong	9.2	6	12.60
Kwantung	74.9	120	102.62
Rest of China and overseas	15.9	11†	21.78
Total	100.0	137	137.00

$$\chi^2 = 11.74 \quad Df = 2 \quad 0.001 < P < 0.01$$

* Chinese population only.

† 7 cases from Shanghai, 3 from Peking and 1 from overseas.

Source: C.I.D. records. Base population data from Hambro Sample Survey.

the rest of China apart from the Province of Kwangtung adjoining Hong Kong, had a significantly lower incidence of S., while those with "native places" in Kwangtung had a significantly higher incidence than would be expected from their numbers in the total Hong Kong population of all ages, assuming that place of origin had no differential effect on the S. tendency.

Persons actually born in Hong Kong naturally had the greatest economic security, with long-established roots, and were psychologically well adapted. These formed only 3 per cent. of the post-war immigrants (Hambro, Table X). Kwangtung people, however (although normally constituting even before the war the greater part of Hong Kong's population), had always been basically attached to their own "native places", remaining in Hong Kong only long enough to make their fortunes in what had long been the main economic centre of the geographical area. These formed as much as 64 per cent. of the post-war immigrants, and among them were many involuntary emigrants from their "native places". Their high S. rate is therefore understandable from the inevitable stresses they had to encounter, and their large numbers among the post-war immigrants was chiefly responsible for the high S. rate of the latter. Persons from other parts of China had a low S. rate, and this may be due to the fact that most of them came from a higher social and economic stratum, i.e., the middle class of northern cities rather than the peasantry of adjoining Kwangtung. As such they were not so much subjected to economic stress, which as we shall later see, was an important precipitating factor in S. This group made up 33 per cent. of the post-war immigrants.

The Factor of Immigration

Relevant data on the origins of 59 S. cases, including the date of their immigration into Hong Kong, were recorded in the dossiers, and these were

taken consecutively as I worked through the latter. There were no special factors related to immigration status that could be regarded as determining in any way the recording or omission of such data in the dossiers, so that the sample could not have been unrepresentative in spite of its smallness.

The bearing of immigration on the incidence of S. is clearly brought out in Table VIII. While the Hong Kong-born and the pre-war immigrants had a

TABLE VIII

Distribution of Suicide by Hong Kong-born, Pre-war Immigrant and Post-war Immigrant Persons

Immigrant Status	Percentage Distribution in Population*		Suicide	
			Observed	Expected
Hong Kong born ..	..	27.0	6	15.93
Pre-war immigrants ..	..	33.1	7	19.53
Post-war immigrants ..	..	39.9	46	23.54
Total	..	100.0	59	59.00

$$\chi^2=35.66 \quad Df=2 \quad P<0.001$$

* A number of 30,000 non-Chinese excluded from the total population.

Source: C.I.D. records. Base population data from Hambro Sample Survey.

similar incidence for S., both rather lower than might be expected from their numbers in the total population (assuming that length of residence in Hong Kong had no effects on the tendency to S.), the incidence for post-war immigrants, most of whom were refugees from the Communist revolution, was much higher than might be expected. The difference is highly significant statistically.

It is necessary, however, to be cautious in assessing the meaning of this finding. The post-war immigrants were on the whole older than the pre-war immigrants and the Hong Kong-born. While persons under 15 amounted to only 35.4 per cent. of the post-war immigrants, they came to 41.6 per cent. of all the others (see Hambro, Table XXV); and persons under 15 are practically immune to S. Furthermore, the M:F ratio (taking all ages) in the former group was 131:100 while in the latter it was only 105:100 (Hambro, *loc. cit.*). The post-war immigrant sex ratio was in fact bigger if those over 14 only were considered. This excess of males must be taken into account since, as we have shown, males are more prone to S. than females.

Nevertheless, it is doubtful if the above facts can explain fully the marked increase in the S. rate for the post-war immigrants. The rates for this group as well as those for the pre-war immigrants and the Hong Kong-born can be calculated from the total number of S. cases recorded for 1954 (302), the distribution of the three groups among these 302 cases according to the data in Table VIII, the proportion of each of the three groups in the population, and the total population for 1954 (2,120,000). The rates per 100,000 total population are then: Hong Kong-born, 5.3; Pre-war Immigrants, 5.1; and Post-war Immigrants, 27.8. It will be noted that the rates for the first two groups are similar to the overall rates for the years before 1949 (see Table I), when the Chinese revolution reached the borders of Hong Kong and the mass influx of political refugees began.

It was possible to obtain a limited amount of further information on the high incidence of S. in post-war immigrants from examination of the C.I.D. records. Among these there were 46 consecutive cases that had data on their actual length of residence in Hong Kong since the end of the war. According

to number of years in Hong Kong, from 1 to 8, these 46 cases of S. fell into the following distribution: 4, 4, 11, 7, 4, 7, 6, 3. This suggests that S. is most likely in immigrants after 3 or 4 years in Hong Kong; and since our cases committed S. between June, 1953 and December, 1954, these persons would have immigrated between 1949 and 1951. It may be that these immigrants passed through a phase of hopelessness and despair after 3 or 4 years, although our data cannot give proof of this. But it is improbable that the actual year of immigration and the events of the period during which these cases committed S. bore no relation to these findings. According to Hambro, as many as 60 per cent. of the immigrants between 1949 and 1951 were political refugees, as compared with only 4 per cent. of those between 1945 and 1947. These political refugees were bound to have been emotionally affected to a great extent by continuing events in their homeland just across the border.

If this was true, then it would be reflected in a rise of the Hong Kong S. rate following closely upon the course of the revolution in China. We can see from Table I that this did in fact happen. There was an increase in the rate in 1949, the year that actual fighting reached Hong Kong's borders. This rise was maintained until 1952, when another marked increase occurred. Following the end of the fighting in 1949, the force of the revolution turned against the land-owners and rich peasants in the land reform movement, which finally reached Kwangtung Province (from which most of the refugees came) in 1952/3. The great dislocation caused by land reform could not but have been a severe stress on the political refugees, many of whom had relatives remaining behind in China who made demands upon them for monetary and other help. The sudden rise in the rate during 1952/3 can thus be understood. Moreover, the strain continued over 1953 and 1954, during which period our cases committed S., owing to the "Three-anti" and the "Five-anti" movements.

Here it might be pointed out that the general effect of war and political tension in lowering the S. rate (previously mentioned in Section A) could not be expected to operate in the case of a community which between 1949 and 1951 received as immigrants some half a million, mostly political, refugees—a number amounting to nearly one-third of the total population estimated for 1950. War and national danger are thought to lower the incidence of S. by inducing the citizen to lose himself in a patriotic cause, by providing full employment and a more definite aim in life: but this clearly cannot apply to a refugee population.

There can be no doubt that the increase in the S. rate since the war has been a real one, and the reason for this is patently related to the fact of immigration from China. That immigrants are exposed to distinct psychological hazards, quite apart from war, is well known. As far as Singapore is concerned, Murphy (1954) found that immigrants, especially the more recent arrivals, had a higher rate than the native-born; and Taff (1951) showed that in Hawaii five out of the six ethnic groups composed mainly of immigrants had much higher rates than the native Hawaiians and that the high rates gradually fell with cultural assimilation. Murphy (1955, p. 182) also showed that East European refugees in Britain had very high S. rates. The fact that immigrants have high rates for mental disorders (Odegard, 1936; Malzberg, 1936) is similarly a reflection of the increased stress they have to encounter.

E. CIVIL STATUS AND FAMILY INTEGRATION

There are unfortunately no data available concerning the distribution of civil status in the Hong Kong population, and in order to obtain control

material for the data relating to civil status in our S. cases it was necessary to carry out a sample survey ourselves.* Although this survey has its limitations arising from sampling as well as its small size, nevertheless it appears sufficiently representative to be used for obtaining a hint, if not more, as to the relation of civil status to S. The attempt to do this may be justified inasmuch as this aspect of the study of S. has, as far as I know, not hitherto been investigated in a non-western culture.

Table IX shows the observed and expected numbers of S.A. in the married, single, widowed, divorced and in concubines, from data available for 211 cases of S. and 159 cases of A.S. In order to assess the significance of the

TABLE IX
Observed and Expected Distribution of Civil Status Groups in Suicide and Attempted Suicide

Civil Status	Suicide				Attempted Suicide			
	Observed		Expected		Observed		Expected	
	M.	F.	Total	Total	M.	F.	Total	Total
Married ..	59	39	98	127.61	34	43	77	96.16
Single ..	53	23	76	61.61	30	36	66	46.43
Widowed ..	4	18	22	20.68	1	10	11	15.58
Divorced ..	4	2	6		—	2	2	
Concubines	—	9	9	1.10	—	3	3	0.83
Total ..	120	91	211	211.00	65	94	159	159.00

Excluding concubines: $\chi^2=12.82$ 13.43
 $0.001 < P < 0.01$ Df=2 $0.001 < P < 0.01$

Source: C.I.D. records and Almoners' histories. Base data from an original survey of civil status in population.

discrepancy between the observed numbers and the numbers expected on the hypothesis that marital status had no specific influence on the tendency to S.A., χ^2 was calculated for the married, the single, and the widowed plus divorced together, but the concubines were ignored because of their small number. It was found that, for S., married persons (of both sexes) were much less numerous than expected, while single persons were more numerous, and the widowed plus divorced rather more numerous. In the case of A.S., married persons were again less numerous, and the single more numerous, but the number of the widowed plus divorced was almost as many as was to be expected.

For the purpose of comparison, S. and A.S. rates for the different civil status groups may be calculated from the numbers of S. and of A.S. in these groups as given in Table IX, and the distribution of these groups in the population over 14 years of age as found in our sample survey. These rates are set out in Figure 4, and they show that for both sexes together the married appear

* After this work had been done, it was discovered that Mr. E. F. Szczepanik, in conducting the sample survey for the Hambro mission, had actually obtained data on the distribution of civil status. These, however, were omitted from the Hambro Report. The data correspond surprisingly well with ours.

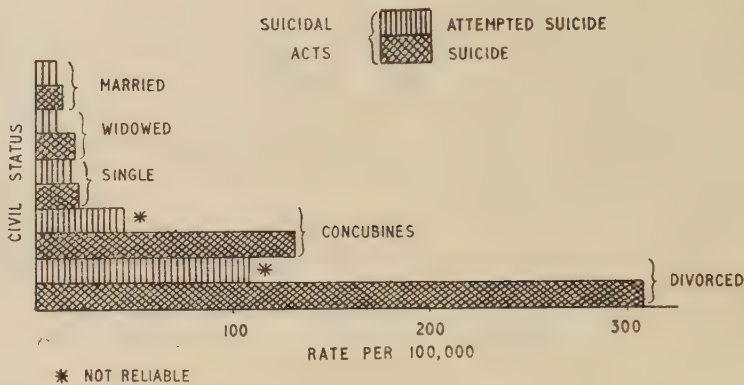


FIG. 4.—Rates for suicidal acts, by civil status (both sexes).

Sources: Almoners' histories and C.I.D. records,

to have the lowest S. rate (12.5 ± 1.30), and then in ascending order the widowed (i.e., including widowers) (17.5 ± 3.73), the single (20.0 ± 2.29), concubines (133.1 ± 44.34) and the divorced (307.7 ± 125.4). Many years ago Durkheim had shown in his classical analysis of this topic that marriage had a protective effect against S. for those over 20 years old (English edition, 1951, pp. 171 ff.). Wicksell (1934) and Dahlberg (1944) have also found this to be true for Sweden, taking all ages into account (quoted by Dahlgren, 1945, p. 53). But in the United States it has generally been found that, when not corrected for age, the single have a lower rate than the married and also the widowed and divorced. (An exception was in Seattle, 1948–52, where the married had the lowest rate, cf. Schmid and van Arsdol, 1955.) However, if age is held constant, the married have the lowest rate (Cavan, 1928, p. 317; Schmid, 1928, p. 373). In London, Sainsbury (1955, p. 65) found that the married as a whole had a higher rate than the single. The low rate we have found for the widowed is unusual, and if valid may perhaps be explained by the existence of the tradition of mutual support associated with the extended family. It is also possible that in Hong Kong there are many young widowed persons, since the span of life here is relatively shorter than in western countries, and it is known that the young are less prone to S.

In the case of A.S., our data suggest that again the married have a low rate (9.8 ± 1.11), but not as low as the widowed (8.8 ± 2.65). The single have a higher rate than the married (17.4 ± 2.14), but the apparently very high rates for concubines and the divorced are not reliable (44.4 ± 25.62 and 102.6 ± 72.49 respectively). The data supplied by Lendrum (1933), showed that the A.S. rate for Detroit, taking all ages and both sexes together, increased from the single to the married and then the divorced, with the widowed having a low rate comparable to the single. Piker (1938) found that widowed women had the lowest A.S. rate too, and widowers a rate only slightly higher than for single or married men; his married persons, if both sexes were combined, had only a slightly higher rate than the single. Dahlgren's figures (1945, p. 53) show that the single had a lower A.S. rate than the married when not corrected for sex or age, and also that widowed persons had the lowest rate. In Seattle, 1948–52, Schmid and van Arsdol (1955) found that the widowed had the lowest A.S. rate and the divorced the highest.

On the whole the findings for Hong Kong do not appear to show any remarkable features either in the case of S. or of A.S., except perhaps for the relatively low S. rate for widowed persons of both sexes. Comparisons however are not very meaningful unless they are made within definite age and sex categories, and our data are not detailed or accurate enough to warrant such analysis. As for the concubines, this group presents certain features of interest, and the reason for their high S. rate will be taken up later when we come to discuss the precipitating causes of S.

Social Isolation

In Table X I have tried to test whether or not there was a significant difference between the incidence of S. in those who were all alone in Hong Kong, without any relatives within the simple conjugal family (i.e., consisting

TABLE X

Observed and Expected Distribution of Cases With and Without Relatives, in Suicide and Attempted Suicide

	Suicide		Attempted Suicide	
	Observed	Expected	Observed	Expected
With relatives	164	170.10	128	128.18
Without relatives	47	40.90	31	30.82
Total	211	211.00	159	159.00
		$\chi^2 = 1.12$		
		$0.20 < P < 0.30$	Df=1	
			0.00	

only of parents and unmarried children), and those who did have such relatives with them. The base data were also obtained in the course of our sample survey of civil status. Although the reliability of this survey may again be questioned, still the results may be of suggestive value at least. Information on the presence or absence of relatives in relation to our S. and A.S. cases came from the usual sources. It must be noted that when the police discover a S. case apparently all alone they put up a public notice asking for relatives, or friends if any, to come forward, so that when later they state in their dossiers that the deceased had or had not relatives, this fact must be taken as reliable. If there were relatives, the relationship was invariably stated. In regard to A.S. the existence or absence of relatives was even less in doubt, since these cases were investigated both by the police and the almoners, whose usual practice was to trace relatives in order to discharge the patients in their care.

It appears from Table X that no significant difference in S.A. exists between the two groups. This may be due to the fact that in Hong Kong social (and economic) support for the individual comes not only from his own immediate family but also from more distant relatives, in keeping with the existence still of the extended family, or at least of customs of mutual aid associated with the latter. The fact that high S. rates occur in socially disorganized areas of cities, where there are much movement, anonymity and especially social isolation of individuals, has been demonstrated by American sociologists (Cavan, 1928, p. 77 ff.; Schmid, 1928, 1937, p. 370 ff.; Mowrer, 1942, p. 347 ff.; Faris, 1948, p. 208 and Schmid and van Arsdol, 1955). The same fact has been demonstrated for London by Sainsbury (1955, p. 78). All these have been ecological studies based on spatially distributed data, and it will be of interest to see if such a method as ours, apparently a more direct one for the problem, can give positive

results in other cities. Doubtless, the concept of social isolation involves a diversity of social and psychological phenomena and means more than merely the idea of individuals living alone without immediate relatives. Perhaps the method here used examines not so much social isolation as family integration alone.

F. OCCUPATION

In 190 cases of S. and 127 cases of A.S. there were available data relating to the occupation (or lack of employment) of the subjects. The rates for various occupational groups are set out in Table XI and presented diagrammatically in Figure 5.

TABLE XI
Suicide and Attempted Suicide by Occupation

Occupation		Percentage in Population*	Suicide		Attempted Suicide	
			No.	Rate†	No.	Rate†
Farmers	..	1.5	1	5.8±0.71	—	—
Fishermen	..	0.5	—	—	—	—
Coolies‡	..	9.5	24	22.1±1.39	8	7.4±0.81
Craftsmen, Cottage	..	7.4	4	4.7±0.64	1	1.2±0.31
Craftsmen, Indep.	..	3.5	3	7.5±0.81	5	12.5±1.04
Labourers, Indust.	..	12.5	15	10.5±0.96	7	4.9±0.66
Hawkers	..	6.5	9	12.1±1.03	5	6.8±0.77
Clerks§	..	5.8	15	22.7±1.41	6	9.0±0.89
Businessmen	..	2.4	3	11.0±0.98	10	36.5±1.79
Professionals	..	3.6	6	14.6±1.13	3	7.3±0.80
Police	..	1.1	—	—	1	7.9±0.83
Others¶	..	5.6	19	29.8±1.62	10	15.6±1.17
Unemployed	..	13.2	56	37.2±1.55	36	23.9±1.42
Housewives	..	26.9	35	11.4±1.00	35	11.4±1.00
Total	..	100.0	190		127	

* Both sexes, excluding those under 15.

† Rate per 100,000 of population aged 15 and over.

‡ Includes amahs.

§ Includes shop assistants.

|| Includes Army.

¶ Includes entertainers and unemployed persons over 60.

Source: C.I.D. records and Almoners' histories. Occupational distribution in the population from Hambro Sample Survey.

It is always misleading to compare crude occupational rates because they are influenced by many factors that are not related to occupation, e.g., age, sex, civil status, and urban or rural environment. Apart from the fact that some of the rates are based on very few cases, their interpretation must be cautious and should take into consideration the nature of the precipitating causes in each group. How important this is may be seen from the fact that among businessmen, for instance, only 1 out of the 3 cases of S. was precipitated by business failure and consequent poverty, and similarly only 3 out of the 10 cases of A.S. One other case of S. and 2 of A.S. were precipitated by causes that could be indirectly related to their occupation, viz., failure to obtain the return of a loan, discovery by the police of a business fraud and the loss by

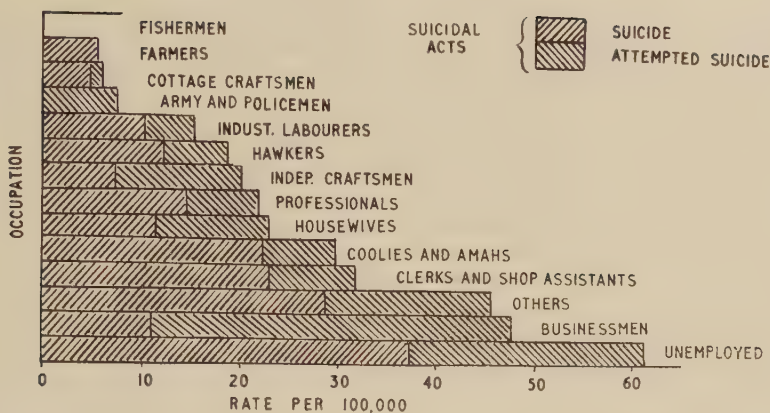


FIG. 5.—Rates for suicidal acts, by occupation (both sexes).

Sources: Almoners' histories and C.I.D. records.

theft of misappropriated money to be used for speculation. But the remaining case of S. was brought on by mental illness, and the remaining 5 cases of A.S. were precipitated by mental and bodily illness, desertion of the wife, and worry over the fate of relatives left behind in China; and all these latter causes were by no means specific to businessmen. We would be wrong therefore to attempt too broad a generalization regarding the S. risk peculiar to a given occupation. However, if we compared only those with the highest and the lowest rates, we could probably identify differences that are truly occupational.

If we take both S. and A.S. together, we find that the highest rates were found for the unemployed, businessmen and "others", and the lowest were found for army men and police, farmers and cottage craftsmen, while fisherfolk did not have a single case. That the rural occupational groups should have low rates is to be expected, for these persons lead lives more secure and less affected by the stresses of culture change; and also they have a certain degree of independence arising from their occupational pursuits, which are broadly based upon the "extended" family, and which satisfy many of their primary needs. The strongly integrated family structure of the fishing community is especially noteworthy. Persons classified under "Army and Police" were almost all policemen, the rest being locally enlisted personnel in the British Army. It is obvious that policemen have much security in their service, like other government servants.

In the case of those occupations with high rates, we find that the common factor among them is economic insecurity, rather than level of income or social prestige. Among "others" were included a variety of occupations that could not be separately considered because the basic population data were too small or unavailable; it included a number of unemployed men and women over 60, a herbalist, a gold speculator (both over 60), barbers, dancing girls, entertainers and prostitutes. The "unemployed" included all those not gainfully occupied, except housewives, students and children (under 15), and none of them were living on unearned income. All these, along with businessmen and entrepreneurs, represent a rather mixed bag, rich or poverty-stricken, socially at contrasting levels; but they were all either economically hard-pressed or were engaged in speculative and uncertain occupations attended by economic risks. In the case of the dancing girls, entertainers and prostitutes, it is well known that many of them are driven to take up such occupations by economic

hardship, often arising from rejection by their family. And also their mode of life is prone to induce in them states of hostile tension because they are often involved in tangled human relationships, or what may be referred to as inter-personal conflict. This group contributed mostly to the impulsive, unsuccessful cases of S.A. classified under "others", in contrast to those over 60, whose attempts were mostly successful, because they were more deliberate.

Businessmen as a group were unusual in that there were among them so many cases of A.S. compared to S. This very low fatality is found in no other group except the army and police, but the latter numbered very few cases. This low fatality is all the more surprising because businessmen are usually of mature age, and therefore might be expected to have a high percentage of "successes" in their S.A. Most studies on the occupational distribution of S. in western countries have been confined to S. rates only and not A.S. The general conclusion has been that merchants, businessmen and professionals have high S. rates (see, e.g., data for England and Wales presented by Dublin and Bunzel, 1933, pp. 94, 399, and by Sainsbury, 1955, p. 19). Murphy (1954) found that in Singapore too this held good. In Hong Kong not only did businessmen have a low fatality, but their S. rate was not high; in fact it was lower than that for coolies and amahs (female domestics) and clerks and shop assistants.

The Influence of Economic Conditions

If the above findings are valid, some explanation must be sought. In themselves the rates for S. and A.S. in this group of businessmen are reliable, being many times their standard errors. It is possible that the low fatality was due to the fact that their S.A. were precipitated not in every case by irrevocable and impersonal financial disaster, but sometimes by other causes, especially those associated with inter-personal conflict, which as we shall see often leads to unconsummated S.A. because either of poor planning and execution or of lack of genuine intent to die. I have already pointed out above the danger of assuming that precipitating causes are always specific to occupation, e.g., financial disaster in businessmen. It is however clear that, generally speaking, businessmen are often involved in situations where S.A. due to morbid hostile tension, with their characteristic impulsivity and lack of deliberation, as well as S.A. with merely an "appeal character", can be expected to occur. This could have been especially true of post-war Hong Kong, with its peculiar economic circumstances in 1953/54.

At that time the embargo against Communist China had obliterated much of the pattern of economic life based on the traditional entrepôt trade, and this had forced many businessmen into highly speculative and unorthodox enterprises that depended a great deal on the human factor for their success. Economic activity was very fluid. At the same time there was a very rapid development of industrial manufacturing. Businessmen (and with them the professional) could still command an income and a means of livelihood if they were resourceful. Social and business conditions were such that the opportunity to try again and perhaps recoup one's losses was always present. These unique conditions could well have accounted for the low S. rate for businessmen as well as the low fatality.

On the other hand, for those with meagre education and only elementary skills, economic conditions were much more unfavourable. There was a superabundance of these persons—the coolies and amahs, clerks and shop assistants—competing for the jobs available, as was to be expected from the doubling of the population within a few years. Their high S. rates are therefore under-

standable. Not only were they poorly paid but their jobs were insecure, and most of the clerks and shop assistants were engaged by old-fashioned Chinese establishments which had none of the welfare provisions that many of the industrial labourers (with a low S. rate) enjoyed. The coolies included many odd-job workers, often unemployed. Actual scrutiny of the main precipitating causes of consummated S. in these two groups of coolies and amahs, and clerks and shop assistants showed that, in about half of the cases in each group, the causes were poverty and illness (including old age), often inter-related.

Rates for Students

In Table XLIII of the Hambro Report, 224, 197 is given for the number of primary, secondary and post-secondary (excluding Hong Kong University) students in Hong Kong in mid-1954. During the period from which we have gathered our S. data there were among students 3 S. and 3 A.S. cases, all of whom were female (none from Hong Kong University). This gives a rate of 1.3 per 100,000 for the group (for S. as well as for A.S.). They were all secondary or post-secondary students, so that if we exclude the 169,067 primary school students from the base population the rates become 5.5. The fact that all the cases were female is of interest, especially in regard to the nature of S. motivation.

G. MAIN PRECIPITATING CAUSES

Much has been written in criticism of attempts to identify the precipitating causes of S. First of all it is evident that the data on which such an attempt is made cannot be first-hand data, but are derived from the reports of police investigators and hospital almoners, who must all have had certain preconceptions about the causation of S. However, these persons are not untrained in the practice of interrogation and in powers of observation, and our almoners all held social science certificates. Moreover, there could have been little that was abstruse in the majority of cases, with their background of poverty, ill health and unemployment, although those cases with a setting of inter-personal conflict, either acute or chronic, might have been more difficult adequately to investigate. Secondly, there is the difficulty that the "causation" of S. (as of mental disease) is a whole process, from which it might be quite misleading to isolate one or two factors. Perhaps it is truer to say that there are causal situations like being poor and disabled by illness, or being poor and unhappily married, leading to the development of the S. impulse.

But even if isolated causal factors, or isolated causes, are not to be regarded as sufficient by themselves, they are necessary causes, and are of more direct significance for the aetiology of S. than all the other sociological factors that we have so far discussed. There are, of course, intimate psychological factors related to the early life of the subject that should also be taken into account, but a complete examination of the aetiology is not practicable and the main immediate or precipitating causes themselves provide useful information and do repay study.

Admitting then a degree of subjectiveness in the data gathered and bearing in mind the need to understand them as inter-dependent rather than isolated, mechanistic factors, we may discuss the main precipitating causes in our series of cases. Tables XII and XIII set out the data in question for both sexes in S.

TABLE XII
Suicide in the Sexes by Main Precipitating Causes

Main Precipitating Cause	Number			Percentage		
	M	F.	Total	M	F.	Total
Insanity	12	5	17	8·8±2·43	6·1±2·64	7·8±1·81
Somatic illness:						
Tuberculosis ..	15	4	19	33·0±4·32	19·5±4·38	27·9±3·04
Other illnesses ..	30	12	42			
Economic:						
Poverty	39	13	52	35·3±4·10	17·1±4·16	28·5±3·06
Business failure ..	2	—	2			
Unemployment ..	7	1	8			
Interpersonal conflict:						
Recent quarrel ..	3	12	15	3·7±1·62	40·2±5·41	17·4±2·57
Parental scolding ..	2	14	16			
Chronic family strife ..	—	4	4			
Desertion	—	1	1			
Unrequited love ..	—	2	2			
Other causes:						
Death of kin	—	3	3	8·9±2·74	9·8±3·29	9·2±1·96
Police action	2	1	3			
Other known	10	4	14			
Unknown causes ..	14	6	20	10·3±2·61	7·3±2·87	9·2±1·96
Total	136	82	218	100·0	100·0	100·0

Source: C.I.D. records.

TABLE XIII
Attempted Suicide in the Sexes by Main Precipitating Causes

Main Precipitating Cause	Number			Percentage		
	M.	F.	Total	M.	F.	Total
Insanity	3	1	4	4·3±2·42	1·1±1·10	2·5±1·24
Somatic illness:						
Tuberculosis ..	5	4	9	10·0±3·58	12·4±3·49	11·3±2·51
Other illnesses ..	2	7	9			
Economic:						
Poverty	18	14	32	47·1±5·97	21·3±4·34	32·7±3·72
Business failure ..	7	1	8			
Unemployment ..	8	4	12			
Interpersonal conflict:						
Recent quarrel ..	3	16	19	22·9±5·02	50·6±5·30	38·4±3·86
Parental scolding ..	2	9	11			
Chronic family strife ..	6	13	19			
Desertion	2	2	4			
Unrequited love ..	3	5	8			
Other causes:						
Death of kin	—	—	—	11·4±3·80	7·9±2·86	9·5±2·32
Police action	—	—	—			
Other known	8	7	15			
Unknown causes ..	3	6	9	4·3±2·42	6·7±2·65	5·6±1·82
Total	70	89	159	100·0	100·0	100·0

Source: Almoners' histories.

and A.S. respectively. There were 218 cases of S. and 159 cases of A.S. for whom data were given in the dossiers, or for whom it was stated that the causes were obscure. These latter cases are classed under "unknown". In the S. cases lack of information concerning causation was due to inability to contact persons sufficiently acquainted with the deceased, but in A.S. it was usually due to lack of co-operation from the subjects or their friends or relatives. It is possible that among the 20 "unknown" cases of S. there were a number who were destitute and all alone, and among the 9 "unknown" A.S. cases were several who had, from indirect information, probably undergone stress from acute inter-personal conflict associated with a certain amount of shame.

Insanity

In classifying cases under the heading of "insanity" I have excluded all those whose mental abnormality has been adjudged to be clearly "reactive" to economic, social or psychological stress, or the result of toxic-infective-

exhaustive illness from organic causes. Thus most of the cases regarded here as insane were probably suffering from brain damage, schizophrenic and paranoid psychoses or severe depressive psychoses. These contributed to only 7.8 per cent. of the S. and 2.5 per cent. of the A.S. cases. "Reactive" mental illness has been excluded because of the importance, from a sociological point of view, of coming to an understanding of precipitating causes. While it is true that differentiation between reactive and endogenous illnesses is often impossible this does not mean that many cases of mental illness are not brought about by traumatic experiences. Since this is so, it is pointless to ignore precipitating causes in the knowledge that, theoretically, there must be also constitutional predisposition.

If, however, we include under "insanity" cases that have been considered to be reactive depression, a total of 44 S. cases could be found in our material, making 20 per cent. of all S. cases; and similarly 9 A.S. cases could be so classified, giving a figure of 6 per cent. of the total. There is in the literature much variation in the findings concerning the incidence of insanity in S.A., doubtless because of different criteria involved and dissimilar circumstances under which the samples chosen for study were obtained. Moreover, the information on which the diagnosis of mental disorder could be made was often inadequate. Our figure for S. is somewhat lower than those found by other workers, but the percentage is comparable to that found by the Metropolitan Life Insurance Company for 1927, and by Cavan in her Chicago studies (*op. cit.*, 1928, p. 112) based on coroners' records, both of which were also 20 per cent. For A.S. the percentage we found is much lower than that given by most authors, but is on the other hand similar to those found by Moore (1937) and Piker (1938) in the United States, both of which were just below 7 per cent. A number of the more important studies on this topic have been summarized by Sainsbury (1955, p. 85).

Economic Stress, Somatic Illness and Inter-personal Conflict

From our point of view, the most interesting findings were that in S. economic stress and somatic illness precipitated slightly more than half (56.4 ± 3.36 per cent.) of all the cases, while inter-personal conflict precipitated somewhat less than a fifth (17.4 ± 2.57 per cent.). On the other hand, in A.S., economic stress and somatic illness were involved in somewhat less than half of the cases (44.0 ± 4.27 per cent.), while inter-personal conflict was involved in well over a third (38.4 ± 3.86 per cent.). The difference between S. and A.S. as regards the frequency of involvement of economic stress and somatic illness is significant (12.4 ± 5.44 per cent.); and similarly the difference between the two as regards the frequency of inter-personal conflict (21.0 ± 9.00 per cent.). There were about twice as many men compared to women who committed S. because of bodily illness, but their numbers in A.S. were similar. Economic pressure precipitated roughly twice as many men as women towards both S. and A.S. But as regards inter-personal conflict, in S. women outnumbered men by more than ten times, and in A.S. by more than twice. All these facts point clearly to one conclusion; that women are impelled to S.A. mainly by psychological difficulties arising from the human environment and not by impersonal stresses due to illness or poverty, whereas in men it is the other way round. This difference is more clearly reflected in S. than in A.S. This finding is important for our understanding of the nature of A.S. It may be pointed out here that the study of Gamble and Burgess (1921) in Peking also revealed findings similar to ours.

It is also clear that inter-personal conflict is less frequently a cause of S. than it is of A.S., again in contrast to economic stress and somatic illness, for men as well as women. Such conflict generally arouses a varying degree of hostile tension in the subject, and produces a state of morbid tension due to suppressed aggression, a condition well-named by Lindemann (1950, pp. 140, 153) as "hypereridism". An attempt at S. in such a state is likely to fail, because of impulsiveness and poor planning as well as inefficient execution.*

Diseases Encountered

In regard to physical diseases, the great importance of tuberculosis (phthisis in all but one case of Pott's Disease) as a precipitating cause is to be noted. This would not be expected in a developed western country. The "other illnesses" involved in S. were: chronic nephritis (3 cases), blindness (3), leprosy (2), cancer of stomach (2), syphilis (2), asthma (2), pleurisy (1), *clonorchis sinensis* infestation (1), chronic malaria (1), painful labour (1), ruptured uterus from labour (1), crippled feet (1), maimed hand (1), chronic skin disease (1), tabes (1) and dysentery (1); in addition to these also enforced withdrawal from narcotic addiction because of poverty (8) and obscure physical illnesses (10). As with tuberculosis, it was found that many of these cases were also associated with poverty, and a number with old age and loneliness. One case of leprosy was in fear of removal to a leper colony, and one case of syphilis had expressed guilt over the infection of his wife.

In regard to A.S., the illnesses involved were as follows: puerperal sub-delirious (toxic-infective) state (2 cases, 1 following a Caesarean operation and 1 with failure to establish lactation), trachoma (1 case, and similarly with each of the following), post-operative adhesions, diabetes, menorrhagia with sterility, peptic ulcer, "headache" and heroin addiction with enforced withdrawal due to poverty. The case of heroin addiction had been trying to obtain admission to hospital in order to wean himself from the drug. Again in the majority of cases the element of poverty entered into the picture, and directly associated with this was the difficulty encountered in obtaining adequate treatment.

Poverty and Unemployment

The factor of poverty, without definite unemployment or recent business failure, was so pervading that it bears further analysis. As regards S., 13 out of the 19 cases of tuberculosis and 22 out of the 42 cases with "other illnesses" were poverty-stricken; and of the 52 cases classed simply under "poverty" 34 were in serious debt, 7 were addicted to narcotics, 6 were inveterate gamblers, and 5 were in positions of dependency on relatives or friends. In regard to A.S., 2 out of the 9 cases of tuberculosis and 3 out of the 9 cases with "other illnesses" were considered to have poverty as a secondary precipitating factor; and of the 32 cases classified simply under "poverty" 26 were recorded as being seriously indebted, 4 were dependent, 1 was an opium addict and 1 a confirmed gambler. Poverty is thus not infrequently associated with illness, drug addiction and gambling (and it is of course closely related to loss of, or inability to obtain, employment). This is more clearly seen in S. than A.S. It may be pointed out in parenthesis that not a single case of chronic alcoholism was found. In Hong Kong narcotic addiction takes the place of alcoholism, though not as regards the immediate precipitation of S.A. (cf. Batchelor's findings in Britain, 1954).

* This topic will be fully discussed in a paper elsewhere.

A number of cases under "insanity" and "somatic illness" were in fact also unemployed, but they were not classified as such because their unemployment was clearly a consequence of their disease. In some of the other categories too the subjects were unemployed, but unemployment was not considered to be the main precipitating cause of their S.A. It is necessary to repeat that our tabulation of "causes" is based on psychological interpretation and insight, and is not a mechanical listing of superficial aetiological factors that may be only doubtfully relevant.

Old Age and Youth

In view of the unusual finding that the Hong Kong female S. rate apparently rises in old age, and that this rate, taking all ages, is relatively so high as to approach the male one (see Fig. 1 and Table I), it is of interest to examine in detail the precipitating causes of S. in persons aged 60 and over. There were in the police dossiers scrutinized 7 male and 7 female cases in this category. All of them were unemployed. The males were all married (although in 2 cases this was not quite certain), and they were all living with their immediate family. Of the females, 4 were widows, 1 married and 1 single, and in the remaining case the information was not available. All these had their immediate family with them, although in 2 this meant only an adopted daughter. Comparison of the main precipitating causes in the two sexes suggests that they were of the same kind, except for the fact that acute quarrels leading to hypereridic states also were to be found in the females. Thus in the males, with the background of poverty in every case, 1 was blind in addition, and 3 were physically ill (2 with phthisis). In the females 2 were poverty-stricken and lonely, 1 had phthisis, and could not get into a hospital, 1 was physically ill and had seen her daughter, who was her sole support, die of phthisis, 1 had found that she had no means of repayment after she had borrowed money to bail out her son from gaol, and the remaining 2 cases followed quarrels, one with her son over being obstructed by the grandchildren and the other with her daughter-in-law over the latter's laziness in the fields.

Only 2 of these cases (one of each sex) were recorded as having been depressed, but there was nothing to suggest that the depression was not reactive to the stresses they had to face. Unfortunately no similar study of the precipitating causes of S. in this age group is known to me that deals with a western country, but there are two studies on A.S. of this kind from Britain (Batchelor, 1953) and the U.S. (O'Neal *et al.*, 1956). Both these revealed a very high incidence of depression and organic deterioration in the subjects, but direct comparison with our findings may be misleading.

It is nevertheless clear that in our series of cases aged 60 and over the factors of penury and chronic physical illness, especially tuberculosis (not normally a disease of old age), were of importance—although the series examined was not large. It is well recognized that poverty and illness fall with especial severity upon the aged. The presence of members of the immediate or conjugal family does not seem to bring with it a protective effect, but rather it may have encouraged S. from altruistic motives. The reason usually adduced for the low S. rate of aged women in the west is that they are given more consideration and succour than men. There is no reason to suppose that this is not also the cultural attitude in Hong Kong, but it is quite likely that the factor of overbearing importance is economic stress, allied perhaps to the breakdown of the "extended" family. It may be noted that the high male and female S. rates in old age are not compatible with the somewhat romantic view often

expressed that old age in Chinese culture is a period relatively free from stress. This at least does not appear to be true in Hong Kong.

Turning now to the younger age groups, we can see from Table XIV that inter-personal conflicts (i.e., from recent quarrel, parental scolding, chronic family strife, desertion of spouse and unrequited love) are far more numerous

TABLE XIV

*Suicide and Attempted Suicide Precipitated by Inter-Personal Conflict and by Economic Stress Plus Somatic Illness, by Age and Sex**

Age	Inter-personal Conflict				Economic Stress and Somatic Illness			
	Suicide		Attempted Suicide		Suicide		Attempted Suicide	
	M.	F.	M.	F.	M.	F.	M.	F.
11-15	..	-	-	1	-	-	-	-
16-20	..	1	6	11	2	1	1	3
21-25	..	1	11	12	16	2	8	10
26-30	..	2	3	5	12	9	8	7
		4	20	15	30	12	17	20
31-35	..	1	5	1	16	4	6	3
36-40	..	-	4	-	14	4	9	2
41-45	..	-	1	-	11	2	4	1
46-50	..	-	-	1	5	4	2	3
51-55	..	-	-	-	9	3	2	1
56-	..	-	3	-	8	1	-	-
		1	13	1	63	18	23	10
Total	..	5	33	16	93	30	40	30
Total M+F		38		61	123		70	
Total S.A. (M+F):								
Below 31				71			79	
Above 30				28			114	

* Cf. Tables XII, XIII and XVIII.

Source: C.I.D. records and Almoners' histories.

as precipitating causes in those under 31 than in those above this age, in contrast to economic stress plus somatic illness, which unlike the former do not produce hypereridism as a rule. The only exception is in female A.S. due to economic stress plus somatic illness, taken by itself, where the usual preponderance of youths in A.S. still holds.

Concubines

Finally, the main precipitating causes in concubines, a youthful group peculiar to Hong Kong and subjected to special psychological stress, may be analysed. It was found that in the 9 cases of S., 4 were childless, so that they had thus lost their *raison d'être* as concubines; and as for precipitating causes in these 4 cases, 2 had been beaten by the principal wife, one refused money by the husband for the Chinese New Year and one had become depressed over her own sterility and therefore her insecure position in the household. Of the remaining 5 cases, 3 had had frequent altercations with the principal wife, one had been denied formal status in the household by the husband who refused to introduce her to the first wife, and one was depressed by the poverty of the family. Of the 3 cases of A.S. in this group, 2 were in long-drawn-out strife with the jealous first wife, and one was trying to eke out a living as a dancer to support 3 children, having been abandoned by the husband. The great importance of inter-personal conflict with strife and quarrelling in this group is clear. In the case of housewives who were not concubines inter-personal conflict of this kind against a background of chronic strife only accounted for a little less than half of the A.S. cases and less than a third of the S. cases. Traditionally, concubines have always been a group with low social prestige (except in ruling families) and we can see in the causes leading to their S. some of the reasons for this.

H. MODE OF SUICIDE AND MORTALITY

Tables XV and XVI show the frequency with which different methods were employed in S.A., S. and A.S., and the mortality of each method according to the sexes. The data are based on the methods employed and their outcome in 1,418 cases between June, 1953 and December, 1954, and were taken from the police records.

TABLE XV

Total Suicidal Acts, Suicide and Attempted Suicide in the Sexes, by Methods Employed

Method	Total Suicidal Acts				Suicide		Attempted Suicide	
	M.		F.		M+F		M+F	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
Jumping from height ..	100	14.8±1.37	52	7.0±0.93	100	28.3±2.40	52	4.9±0.66
Drowning ..	138	20.5±1.55	128	17.2±1.38	36	10.3±1.62	230	21.6±1.26
Hanging ..	75	11.2±1.21	62	8.3±1.01	106	30.0±2.44	31	2.9±0.51
Wounding ..	39	5.8±0.90	23	3.1±0.63	8	2.3±0.79	54	5.0±0.66
Hypnotics ..	34	5.0±0.79	41	5.7±0.81	—	—	—	—
Lye ..	154	27.9±1.73	224	35.6±1.76	55	15.6±1.92	398	37.4±1.48
Other and unknown								
poisons ..	95	14.2±1.34	130	17.5±1.39	41	11.6±1.70	184	17.3±1.16
Other methods ..	24	3.4±0.70	46	6.2±0.88	3	0.8±0.48	67	6.3±0.74
Unknown methods ..	15	2.2±0.56	38	5.1±0.81	4	1.1±0.56	49	4.6±0.64
Total ..	674	100.0	744	100.0	353	100.0	1,065	100.0

Source: C.I.D. records.

TABLE XVI

Methods of Suicide and Outcome by Sex

Method	Suicidal Acts			Mortality Percentage		
	M.	F.	Total	M.	F.	Total
Jumping from height ..	100	52	152	70.0±4.58	57.7±6.85	65.8±3.85
Drowning ..	138	128	266	15.2±3.06	11.7±2.84	13.5±2.10
Hanging ..	75	62	137	85.2±4.10	67.7±5.94	77.4±3.58
Wounding ..	39	23	62	7.7±4.27	21.7±8.59	12.9±4.26
Hypnotics ..	34	41	75	17.6±6.53	9.8±4.53	13.3±3.92
Lye ..	154	224	378	13.6±2.76	10.7±2.06	11.9±1.66
Other poisons ..	60	92	152	13.3±4.38	13.0±3.51	13.2±2.74
Unknown poisons ..	35	38	73	28.5±7.63	28.9±7.35	28.8±5.30
Swallowing objects ..	4	19	23	0.0	0.0	0.0
Jumping before train ..	1	—	1	100.0	—	100.0
Electrocution ..	1	—	1	100.0	—	100.0
Battering head ..	5	—	5	0.0	—	0.0
Gun-shot ..	1	—	1	100.0	—	100.0
Other methods ..	12	27	39	0.0	0.0	0.0
Unknown methods ..	15	38	53	13.3±8.76	5.3±3.63	7.6±3.64
Total ..	674	744	1,418			

Source: C.I.D. records.

It can be seen that “active” methods with a high mortality, viz., hanging and jumping from a height, were common, and were used in S.A. by more men than women. Jumping into the sea was another common method and was used in S.A. by more men than women, but the difference was not great, and the method had a low mortality. The other common method was poisoning with hypnotics and lye (caustic soda), but this had a low mortality and had a preponderance of women employing it. The remaining methods—wounding, use of other and unknown poisons, and those subsumed under “other”—were not common, had comparatively low mortality rates, and were more often employed by women. This being so, with women commonly using methods of low mortality, we would expect that in S.A. as a whole females should have a lower mortality rate than men.

Sex Differences in Mortality

Analysis shows that the sex difference in mortality was actually what might have been expected. Of the men, 208 out of 674, or 30.9 ± 1.78 per cent. died; and of the women, 145 out of 744, or 19.5 ± 1.46 per cent. succumbed. The difference between the two percentages was 11.4 ± 0.83 per cent., which is highly significant. For the sake of completeness it may be added that the mortality for both sexes taken together was 24.9 ± 1.15 per cent. This rate is lower than that found by Dahlgren for his series (55.0 per cent.) and also that obtained by Schmid and van Arsdol (30.7 per cent.) for Malmoe and Seattle respectively (Dahlgren, 1945, p. 71; Schmid and van Arsdol, 1955). It is however always of dubious value to make these comparisons for we can never be quite sure that all A.S. cases have been reported and therefore included in the figures from which the mortality rate is derived. Nevertheless, in regard to the lower mortality rate of women, this finding appears to be universal and has been observed also in Asian populations (cf. Anzures, 1927, for Manila; Murphy, 1954, for Singapore; the W.H.O. survey, 1956, for Ceylon, Japan, and also various western countries; Dublin and Bunzel, 1933, p. 54; Dahlgren, 1945, p. 71 and Schmid and van Arsdol, 1955).

It does not appear sufficient to ascribe the lower fatality rate from S.A. in women only to the fact that they employ more often the methods with a lower mortality (or lower efficiency), since Table XVI shows that for every method men had a higher mortality than women, except for "wounding", while "other poisons" and "unknown poisons" were more or less equal. The male mortality rate for "wounding" (7.7 ± 4.27 per cent.) is however not valid, so that a comparison with the corresponding female rate is pointless. We may reasonably conclude therefore that masculinity in itself, with its vigour and determination, is a factor tending to make S.A. successful, whatever the method used. This is often ignored in discussions of the topic, but it is also borne out by Lendrum's material (*op. cit.*, 1933). Similarly Schmid and van Arsdol (1955) found that male mortality exceeded the female in every method except "cutting". Such forcefulness and determination is of course also to be expected in the mature compared to the young. It is to be noted that this factor operates not only in "active" methods like jumping from a height, drowning and hanging, but also in poisoning by hypnotics and lye, for more lethal quantities may be ingested to make the outcome certain. In the case of "other poisons" no such factor appears involved because they included extremely poisonous substances like arsenic and cyanide, or substances that were fanciful rather than strongly poisonous, such as liquid tooth paste and hair lotions.

It may also be recalled here that the mortality is lower in the young than the old, since the S. : A.S. ratio is smaller in the former, a finding which is usually the rule (see Section C).

To say that a S.A. fails because the method employed is not lethal is of course to some extent tautological. It is necessary to explain why such a method is chosen to begin with, and here we should beware of falling into an intellectualistic interpretation of the subject's choice of a method. This might lead us erroneously to say that because the S. failed or because the method chosen was inefficient, therefore the subject was not really serious about the intention to die (cf., for example, Murphy, 1954). The choice of method depends on its availability, on whether it is within immediate physical reach, on the knowledge, experience and maturity of the subject, on suggestion and learning, and on the subject's mental state at the time. The last is of great importance, and involves essentially the question whether or not the subject was in a position coolly

and efficiently to plan and execute his desire to end his life, without being rent by hostile tension as a result of inter-personal conflict.

I. MONTHLY DISTRIBUTION

It was possible from the records of the Registry of Births and Deaths to obtain the monthly totals of S. cases for the years 1948-1955, the figures for 1953 and 1954 having unfortunately not been entered by the month. In Table XVII I have set out the actual numbers of S. for the different months, the

TABLE XVII

Monthly Distribution of Suicide Cases (1948-52, 1955) and of Mental Hospital Admissions (1948-55)

Month	Absolute Figures		Reduced* and Expressed per 1,000		Mean of 3 Consecutive Months	
	S.	M.H. Admission	S.	M.H. Admission	S.	M.H. Admission
January	70	506	68.4	83.0	72.1	86.4
February	66	485	70.8	86.0	75.1	87.9
March	88	583	86.0	94.7	76.9	88.9
April	73	511	73.8	86.0	83.3	91.9
May	93	600	90.9	95.2	89.4	88.7
June	102	505	103.2	85.0	93.5	82.7
July	88	418	86.3	67.9	92.4	77.3
August	89	484	87.4	79.0	88.9	74.3
September	92	452	93.0	76.1	87.5	77.2
October	84	470	82.1	76.4	85.3	77.8
November	80	478	80.9	80.4	80.1	82.4
December	79	556	77.2	90.3	75.5	84.5
Total	1,004	6,048	1,000.0	1,000.0	1,000.0	1,000.0

* Reduced according to the length of the different months to a standard 30-day month.

Source: Registrar of Births and Deaths and Hong Kong Mental Hospital.

material comprising 1,004 cases for the 6 years. The figures obtained after the monthly numbers have been reduced according to the varying lengths of the months to standard 30-day months have been calculated and, for the sake of comparison, these numbers have been expressed per 1,000 cases and placed alongside similarly computed figures derived from the monthly admissions to the Hong Kong Mental Hospital for the years 1948-55 inclusive. Furthermore, in order to present the data from the two sources graphically, the means of 3 consecutive months have been calculated for each month and the curves for these figures plotted in Figure 6.

The S. curve shows a peak in June, and this is similar to what has been found for the majority of European countries in which this topic has been investigated, where the maximum incidence of S. is in spring or early summer (cf. Dublin and Bunzel, 1933, p. 86; Dahlgren, 1945, p. 58; and Sainsbury, 1955, p. 67). Hong Kong, although lying just within the Tropics, has a distinct change of seasons with a moderately cold winter. In European countries, furthermore, a smaller peak is often found in the autumn. Our curve does not reveal this, but the "unsmoothed" figures in Table XVII indicate such a rise in September. Various authors have noted that the curves for mental hospital admissions parallel that for S. (cf. Dahlgren, 1945, p. 61), but this does not appear to be true for Hong Kong.

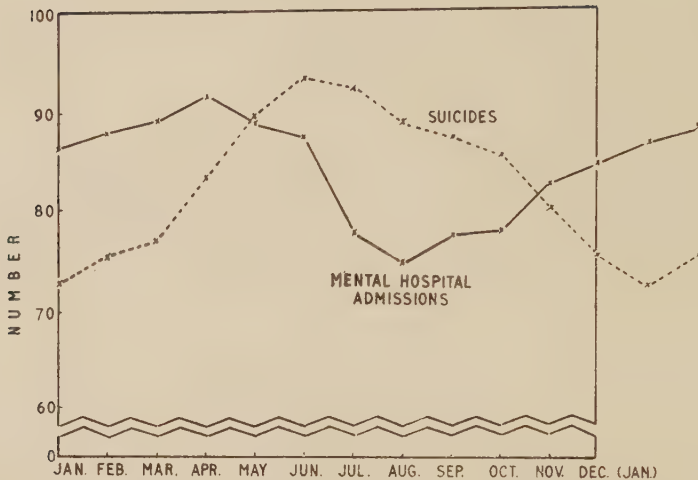


FIG. 6.—Monthly distribution of suicides (1948–52, 1955) and Mental Hospital admissions (1948–55) in Hong Kong.

(Figures are the means for 3 consecutive months: see Table XVII.)

Sources: Registry of Births and Deaths and Mental Hospital.

IV.—CONCLUSION

Most of the facts that have been demonstrated about S. in Hong Kong fit in with findings from other countries, but there are some unusual results emerging from our enquiry which are probably related to the social, economic and cultural *milieu* of the Colony.

Of socio-economic significance are the rise in the S. rate in recent years, the high rate in post-war immigrants and the fact that the highest rates were found among low-income and low-prestige groups (clerks and shop assistants, coolies and amahs) rather than among the rich and socially elevated. These facts, taken together with the finding that, as in other countries, the urban rate is higher than the rural, would seem to indicate that the processes of urbanization and industrialization, with the breakdown of primary group support and the substitution of modern values for the traditional, have led to an increase in S.

It is possible to invoke the concept of “social disorganization” to account for such findings, as has been demonstrated by several sociologists. “Social disorganization”, according to the definition given by Faris, is “disruption of the functional relations among persons to a degree that interferes with the performance of the accepted tasks of the group” (*op. cit.*, 1948, p. 19). Different processes contributing to such disorganization, which has also its psychological consequences, have been studied: e.g., varying speed of social and cultural changes in different segments of a society (Bloch, 1952, pp. 16 ff.), urbanization and industrialization (Mayo, 1949, pp. 7 ff.), and disturbance in the pattern of economic life, or essentially of the production and distribution of human necessities (Faris, 1948, pp. 69 ff.). The relation of S. to various aspects of social disorganization has been convincingly demonstrated by Durkheim (1951, pp. 241 ff.), whose original concept of “anomic” S. from the failure of the individual to adjust to social change inspired so much of the later work in this field, e.g., those by Cavan (1928, pp. 77 ff.), Halbwachs (1930), Mowrer (1942, pp. 347 ff.), Faris (1948, pp. 207 ff.) and Sainsbury (1955, pp. 75 f.).

However, although the significance of social disorganization in raising the S. rate cannot be doubted, emphasis on this may lead to neglect of the very important and direct influence exercised in Hong Kong by acute political stresses and violent economic dislocation, and the enormous influx of refugees—or involuntary immigrants—fleeing from a revolution. It would be unrealistic not to scrutinize such factors for their ultimate psychological significance, and lose oneself in an abstract concept like “social disorganization” in this study.

An important complex of factors arising from unemployment, poverty and chronic physical illness (which last often leads to penury through unemployment) has been shown to be a major precipitating cause of S. Dublin and Bunzel (1933, pp. 96 ff.) noted that while high S. rates occurred among those at the upper end of the social and economic scale, among the labouring class it was those nearest the poverty line who most frequently committed S. Sainsbury (1955, pp. 72 ff.) found that although high rates were found for the richer London boroughs, among the S. cases in these boroughs were many who were actually living in poverty. The general conclusion has been that it is actually the change from affluence to poverty that precipitates S. This, however, need not be true in communities where many live at a marginal economic level and are plunged into stark destitution with unemployment or chronic illness. We have seen that in Hong Kong high rates occurred among the low income group rather than the high.

It is also possible that among the low income group the psychological effects of urbanization have been more deleterious, since they have been more recently and forcibly wrenched from their village and rural background. Psychological factors, apart from the social and economic, have been demonstrated to be of great significance in enabling us to understand the special characteristics of S. in Hong Kong, viz., the high female S. rate, which is more than half the male; the relatively numerous cases of A.S. compared to S., especially among the young of both sexes; and the apparent rise in the female S. and A.S. rates after 60. The best explanation for these features is to be found in the psychological influence on the young, especially young women, of their own cultural background. Hong Kong women are in general subordinate to men at all ages, and do not possess much freedom in organizing their own lives. Patriarchal traditions still obtain and the chances of economic independence for women are still restricted, though increasingly less so, with industrialization advancing apace.

In these circumstances, conflict arising from the human environment, or inter-personal conflict, when it involves persons in culturally defined positions which do not permit them directly to retaliate against those who aggrieve or frustrate them, will lead to hypereridic reactions. These precipitate, or rather issue in, a large number of S.A. In this connection it is of interest to note that the Penal Code of the Ching Dynasty in China (which preceded the Republic) gave explicit recognition over the course of several centuries to S. arising from shame, rage and excitement; moreover, incitement to S. was punishable like manslaughter, so that there was cultural support for the idea of S. as an indirect form of revenge against one's provocators (cf. Alabaster, 1899, pp. 303 ff.).

The whole question of the influence of the cultural background on S. is a complex one, into which we cannot fully go. The Buddhist belief in metempsychosis,* and also the belief in an after-life associated with ancestor-worship, must have a bearing on the proclivity to S. We have to draw attention to one

* This specialized topic in relation to S. has been the subject of an enquiry by J. Evola (1955).

important fact at least, and this is that standardized and culturally sanctioned S. does not exist among modern Chinese. This is in contrast to the Japanese, who have the highest female S. rate in relation to the male (cf. the W.H.O. survey, 1956). In the past, however, culturally approved S. comparable to those existing in present-day Japan occurred (cf. Maclagan, 1922, XII, pp. 26 ff.). The comparatively high rate found in Chinese females must therefore be attributed to disabilities arising from the status of women, and not to social sanction.

SUMMARY

An analysis has been made of suicide in Hong Kong as regards trends in time, age, sex, certain sociological variables, main precipitating causes, methods employed, mortality and seasonal distribution. Many of the findings are in keeping with what has been demonstrated in western communities, but, while "social disorganization" can be invoked to explain certain results, the direct influence of social and economic disturbance arising from events in adjoining China should not be overlooked.

The unusual findings of a high female rate compared to the male, of a noticeably large number of unconsummated suicidal attempts in the young, as well as the rise in the female rate in old age, are probably due to certain psychological factors arising from the subordinate status accorded to women and to young persons. Changing age and sex trends in the Chinese suicide rate can be demonstrated, and are thought to be related to the dissolution and modernization of a traditionally patriarchal culture.

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BIBLIOGRAPHY

- ALABASTER, E., *Notes and Commentaries on Chinese Criminal Law*, 1899, pp. 303ff. London.
 ANZURES, P., *J. Phil. Is. med. Ass.*, 1927, 7, 235.
 BATCHELOR, I. R. C., *Brit. med. J.*, 1953, ii, 1186.
Idem, *J. Ment. Sci.*, 1954, 100, 451.
 BLOCH, H. A., *Disorganization, Personal and Social*, 1952, pp. 16ff. New York.
 BOND, H., *Brit. med. J.*, 1931, ii, 234.
 CAVAN, R. S., *Suicide*, 1928, pp. 36, 77ff., 112, 314, 317. Chicago.
 CHEN, TA, *Population in Modern China*, 1946, pp. 100, 101. Chicago.
 DAHLBERG, G., *Svenska Lakartidn.*, 1944, 41, 1649. Quoted by Dahlgren (1945), p. 53.
 DAHLGREN, K. G., *On Suicide and Attempted Suicide*, 1945, pp. 43, 51, 53, 58, 61, 71, 145ff., 173ff., 219ff., 221. Lund.
 DUBLIN, L. I., and BUNZEL, B., *To Be Or Not To Be*, 1933, pp. 45, 54, 78, 86, 94, 96ff., 101, 382, 399, 415. New York.
 DURKHEIM, E., *Suicide*, 1951, pp. 171ff., 241ff. Glencoe, Ill. (Original French Edition, Paris, 1897.)
 ELLENBERGER, H., *Mshr. Psychiat. Neurol.*, 1953, 125, 347. Quoted by Strauss (1956).
 EVOLA, J., *East and West (Rome)*, 1955, Year 6, No. 1, p. 41.
 FARIS, R. E. L., *Social Disorganization*, 1948, pp. 19, 69ff., 207, 208, 213. New York.
 GAMBLE, S. A., and BURGESS, J. S., *Pekin: a Social Survey*, 1921, pp. 116, 117, 416. New York.
 GUNASEKARA, N. D., *J. Ceylon Br. Brit. med. Ass.*, 1951, 46, 1.
 HALBWACHS, M., *Les Causes du Suicide*, 1930, pp. 202, 214. Paris. Quoted by Faris (1948).
 HAMBRO, E., *The Problem of Chinese Refugees in Hong Kong*, 1955, pp. 17, 18 and Tables III, X, XI, XXV, XXXI and XLIII. Leyden.
 HSU, F. L. K., *Americans and Chinese*, 1953, p. 63. New York.
 LAMBO, T. A., *Brit. med. J.*, 1956, ii, 1388.
 LENDRUM, F. C., *Amer. J. Psychiat.*, 1953, 90, 479.
 LINDEMANN, E. R., *Epidemiology of Mental Disorder*, 1950, pp. 140, 141, 153. New York.
 MACLAGAN, P. J., "Suicide (Chinese)", in *Encyclopaedia of Religion and Ethics*, 1922, XII, pp. 26ff. New York.
 MALZBERG, B., *Amer. J. Psychiat.*, 1936, 93, 127.
 MAYO, E., *The Social Problems of an Industrial Civilization*, 1949, pp. 7ff. New York.
 MENNINGER, K. A., *Man Against Himself*, 1938, pp. 24ff. New York.
 METROPOLITAN LIFE INSURANCE CO., *Statistical Bulletin*, 1927, VIII, No. 4, p. 4. New York.
 Quoted by Sainsbury (1955), p. 85.
 MOORE, M., *New Engl. J. Med.*, 1937, 217, 291.
 MOWRE, E. R., *Epidemiology, Personal and Social*, 1942, pp. 339, 343, 347ff. Philadelphia.
 MURPHY, H. B. M., *Med. J. Malaya*, 1954, 9, 1.
Idem, *Flight and Resettlement*, 1955, pp. 182ff. Paris.

- ODEGAARD, O., *Mept. Hyg.*, 1936, **20**, 546.
- O'NEAL, P., ROBINS, E., and SCHMIDT, E. H., *Arch. Neurol. Psychiat.*, 1956, **75**, 275.
- PIKER, P., *Amer. J. Psychiat.*, 1938, **95**, 97.
- SAINSBURY, P., *Suicide in London*, 1955, pp. 19, 65, 67, 72ff., 75ff., 85, 86. London.
- SCHMID, C. F., *Suicide in Seattle 1914-1925*, 1928, p. 373. Seattle. Quoted by Sainsbury (1955), p. 18.
- Idem*, *Amer. J. Sociol.*, 1933, **39**, 30.
- Idem*, *Social Saga of Two Cities*, 1937, pp. 370ff. Minneapolis.
- Idem* and VAN ARSDOL, M. D., JR., *Amer. Sociol. Rev.*, 1955, **20**, 273.
- SCHROEDER, W. W., and BEEGLE, J. A., *Rural Sociol.*, 1953, **18**, 45.
- STRAUSS, E. B., *Brit. med. J.*, 1956, *ii*, 818.
- STRAUSS, J. H., and STRAUSS, M. A., *Amer. J. Sociol.*, 1953, **58**, 461.
- SZCZEPANIK, E. F., *Far Eastern Econ. Rev.*, 1955, **19**, 385.
- TAFF, M. A., JR., *Suicides in Hawaii, 1941-50*, 1951. Honolulu.
- WICKSELL, S., *Svenska Förenigens för psykisk hälsovård smaskrifter nr 7*, 1934. Stockholm.
- Quoted by Dahlgren (1945), p. 53.
- WILE, I. S., *Amer. J. Orthopsychiat.*, 1937, **7**, 235.
- WORLD HEALTH ORGANIZATION, "Mortality from Suicide", *Epidem. and Vital Statistics Report*, 1956, **9**, No. 4, pp. 243ff., 250ff., 275ff. Geneva.
- ZILBOORG, G., *Amer. J. Psychiat.*, 1936, **92**, 1347.
- Idem*, *Amer. J. Orthopsychiat.*, 1937, **7**, 15.

THE CASE OF MR. KOVISH*

By

HUMPHRY OSMOND, M.R.C.S., D.P.M.

Physician Superintendent

Saskatchewan Hospital, Weyburn, Saskatchewan

and

ABRAM HOFFER, Ph.D., M.D.

Director, Psychiatric Research

University Hospital, Saskatoon

INTRODUCTION

NOWADAYS when clinicians feel happier guided by methodologists and statisticians, only the brash or the sophisticated care to report single cases in detail. Yet medicine has in the past profited greatly by careful observations of this sort, and so good is this precedent that we have decided to defy fashion and report the affair of Mr. Kovish, one of those curious happenings which, we think, deserves a permanent record.

Some months ago we received a rather diffident letter from a Mr. Kovish of Chicago, suggesting that we might be interested in a recent experience of his. He had been an asthmatic for many years who took adrenalin regularly by inhalation and was well acquainted with its effect. Some time before writing to us he had bought some adrenalin of a wine colour and after taking it had suffered about four weeks of wholly unaccustomed mental distress. His account of this was so vivid that we recognized immediately that he was an accurate observer with an inquiring and reflective mind, who had endured what was for him a uniquely unpleasant experience. Further, he was knowledgeable enough to realize that such an experience carefully recorded might be very useful to those who research into the relationship between schizophrenic illnesses and those model psychoses which can be produced by chemical and other means.

THE NATURE OF THE DATA

While we had the advantage of a co-operative and intelligent subject who was able to give a clear and detached account of his experience, limitations are imposed by someone who has not been under medical care, who has volunteered help and who lives a long way off. As soon as we heard from Mr. Kovish we wrote asking him to send us a full and exact account of what had happened to him. He supplied us with this in a series of letters and a carefully prepared schedule. His first letter was written about two months after the episode which we shall describe occurred. Then by good luck in early November, 1956, business took us to New York and on the day of the presidential election, Mr. Kovish flew in to join us. He spent nine hours with us. We had two meals with him, recorded nearly an hour of questioning on a dictaphone and at the end of the meeting we each recorded our impression. We sent him a transcript of our recording and asked for his comments. We received letters from his wife and a friend who plays a large part in the story.

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Early in 1957, we were able to make separate visits to Mr. Kovish's home town, and each of us spent most of two days with him, seeing him in his home and at his place of business. We questioned his wife, his brother, his friend, and saw him with his children and with colleagues. So we have been able to compare the account that he had given of himself in his letters and in New York with what we saw on the spot. We feel that we have checked his statement thoroughly and so far as clinical evidence in psychiatry can be accurate, this is. We concluded that he is an unusually successful young businessman, much above average in intelligence and alertness, who by his mid-thirties has worked himself up from poverty to substantial affluence. We did not consider that he was engaged in a publicity-seeking prank or suffering from a delusional illness. Our method prevented us from undertaking many procedures usually thought necessary in psychiatric examination. But, on balance we believe that by denying ourselves formal investigations and tests of various sorts we have gained more than we lost.

In addition to information collected from Mr. Kovish, we have been able through the kindness of the Connaught Laboratories, Toronto and the Parke, Davis Company of Canada, to examine over 500 bottles of the 1 per cent. adrenalin solution used by asthmatics—every bottle had been returned to them because it was discoloured.

We have further made a small number of personal experiments to see whether we could throw any light on Mr. Kovish's story. These experiments, while not conclusive, are we think suggestive.

Our data then are mainly descriptive and biographical. They have been collected and checked with care. We hope that we succeeded in doing this without wearying our subject who was most generous with his time and hospitality.

LETTER FROM MR. KOVISH DATED JULY, 1956

"My interest in your article is a personal one as it deals with an experience which I endured and which may be of some interest to you. I have had asthma and have used an adrenalin spray for the past 15 years (1:100 in strength). For the past seven years I have usually only had to use the spray at night before going to bed. I have noticed no unfavourable effects from its use except that about 10 years ago I used some discoloured adrenalin and had headaches and became dizzy. I have always felt myself to be a normal individual—unneurotic and with a zest for living. It was therefore quite a shock to me to find one day that I had suddenly become an individual who: (1) saw the world as through a distorted glass—I sought to interpret the visual distortions as being due to strange mental processes, (2) became quite anxious and depressive, (3) had compulsive thoughts, and (4) began to doubt myself and my sanity. I became quite panicky for I had never experienced anything even remotely like these feelings. They were alien to me and I had nothing to compare them to. I could only conclude that something had gone wrong with me psychologically. However, because I am very organically oriented, I felt for a few fleeting moments that since I had never had any of these strange feelings before (in fact, they were diametrically opposite to the feelings which I had always had), there might be a physiological basis. The only drug that I had taken was adrenalin and I recalled that this had all started one night about two months ago when I had been travelling and needed adrenalin. I stopped at a drug store and the druggist said that all he had was one bottle of adrenalin but that it was quite discoloured; he seemed hesitant to sell it to me but I bought it anyway. I was able to fix the day all my unrest started as the day I used the discoloured adrenalin. However,

I dismissed this from my mind as being pure coincidence and felt that I was just rationalizing and that I was actually having a mental 'breakdown'. This threw me into a panic and I spiralled downward and was getting progressively worse. (In the meantime I kept taking the 'bad' adrenalin for about a month at various intervals until I had finished the bottle.) I was thoroughly frightened and ashamed at what was happening to me as I attached a stigma to mental imbalance. I think I should point out that my off thoughts, discomforts, etc. were all internal and that my associates did not comment on any difference in my behaviour; however, internally I was in a state of terrific strain and almost collapsed at times.

"About three weeks ago I felt so bad that I discussed this with a botanist friend of mine, who knows me quite well. She would not agree that psychologically I could have so drastically changed, and asked me if I could think of any physiological cause to lay this to. When I casually mentioned the adrenalin, my friend immediately reminded me of Gerard's article in last August's *Science* on 'Biological Roots of Psychiatry' in which he discussed breakdown products of adrenalin as causing temporary psychoses. In re-reading Gerard's article I found a description that seemed to fit my feelings exactly. I wrote Dr. — for further information on this subject and he referred me to your article.

"I became better—due mostly to the insight into the cause of my disturbance. I felt that the adrenalin effects would wear off, hoping that none of them were irreversible. However, while my mental outlook progressed to normalcy, I still felt physiologically different: (1) I had always been a sound sleeper. Now I developed insomnia, easy arousal, and strange hypnagogic effects before sleep came. (2) I seemed to have a dull, anxious feeling inside me. (3) There was heightened nervous attention to my surroundings. (4) One of the strangest occurrences was that despite my lack of sleep and my not taking adrenalin, I had no asthma and felt almost as if I had just taken adrenalin.

"I began to think that I had been using the adrenalin as a handy handle to lay my troubles to—because it would seem that after several weeks the adrenalin should have been removed from my system. Was I actually becoming a neurotic? This again caused me to feel panicky. Forcing myself to look at this scientifically, I wondered if the adrenochrome might have triggered off a metabolic change, affecting my own adrenal functioning and causing continuing disturbance. I looked up your article and found it extremely interesting as it describes in so many ways my own feelings during the episode.

"Had I known that I was going to have an artificially-induced psychosis, I am sure the severity of the consequences would not have been felt because, not knowing the origin, I ascribed it to psychic difficulties and this led me deeper into my feelings of unrest, I went into a tailspin, and perhaps this had something to do with the length of the effect. I also should mention that I was in a personally stressful situation which might have been a contributing factor."

Table Prepared by Mr. Kovish, September, 1956

Time	Symptoms	Comments
Sunday, 20 May (11 p.m.)	Before adrenalin felt fine, very normal. After, vividness of surroundings.	Took adrenalin (Epinephrine 1:100 inhalation Parke, Davis) of wine (grape) colour with definite amber tint. Took it several times through the evening. I recall inhaling through the nose as well as through the mouth.

Time	Symptoms	Comments
Monday, 21 May	Unusually awake. Difficulty in judgment in driving. Some bizarre thoughts. Distorted vision, no people on the road.	Took it again in the morning at approximately four o'clock.
Tuesday, 22 May	Tendency toward fixed thinking. Feelings of excitement for no apparent reason. Surroundings especially people (men much more so than women) looked peculiar, including pictures in newspapers.	Took adrenalin probably twice; in the evening and in the morning with a possibility of taking it in the middle of the night.
Wednesday, 23 May	Some anxiety. Visual sensitivity which appeared somehow as distortions. I believe there were some fixed strange thoughts.	Adrenalin again two or three times.
Thursday, 24 May	Great personal shock. Accident to friend. Mounting anxieties and unrealness.	Anxiety has never been a noticeable part of my personality. I can never recollect anxiety ever in the form that it was developing.
Friday, 25 May through Sunday, 27 May	Anxieties and internal excitement. Visual sensitivity and distortions, strangeness. Compulsive thoughts recognized as not being based on reality therefore considered delusory.	Adrenalin approximately three times a day. The taking of adrenalin (Epinephrine) was a matter of habit. I would take it before sleep in order to assure the wheezing did not disturb me. If I woke up in the middle of the night I would take it again as a matter of habit, and sometimes in the morning I would do the same. Intake therefore is approximately the same as the first night.
Sunday, 27 May through Thursday, 31 May	Anxiety. Depression. Compulsive thoughts. No relation to reality. Some panic realizing I was not able to shake the bizarre thoughts. Thoughts seemed to be connected with the visual appearance of people.	I spoke about it to a friend (due to the nature of the thoughts, see detailed report). It was dismissed as of no importance and possibly connected to the anxiety-producing situation that I was involved in. Adrenalin (Epinephrine) intake approximately the same as before.
Thursday, 31 May to Tuesday, 5 June	Increasing tension. Increasing noticeability of strangeness of visual reactions. Increasing panic. Increasing compulsive thinking. I reproached myself for all the things I had confidence and belief in, my work, etc.	The panic element was increasing greatly. As it was evident that I could not wilfully get rid of the strange feelings that I was experiencing. I was definite that there was something wrong with me mentally which frightened me. The more frightened I got, the worse it became.
Tuesday, 5 June (Flew to Florida)	Fear. Threatening and foreboding feelings. Groups of people accelerated my attention to surroundings that I never had had. Everything appearing to be as never seen before. Extreme anxiety. Was bothered by it while flying. Flying had never bothered me through any storms, or other anxiety-producing situation and had never bothered me previously.	Adrenalin intake the same. <i>Note:</i> Somewhere, I cannot recall the time, I purchased another bottle of adrenalin and used part of it. Still seemed to be taking adrenalin as a habit although I did not seem to need it as I had before.

Time	Symptoms	Comments
Wednesday, 6 June and Thursday, 7 June	Continued anxiety and agitation.	I noticed that in the movies the figures on the screen seemed to increase my discomfort.
Friday, 8 June	Extreme anxiety, almost unbearable. Agitation due to continuing feelings that I could not stop the situation and return to normal.	Still taking adrenalin.
Returned from Florida		
From Friday, 8 June to Wednesday, 27 June	Recollection unclear of detailed dates. Anxiety. Tension. Visual strangeness. Depression. Compulsive internal thoughts.	Still taking adrenalin.
Wednesday, 27 June	This was an extremely bad day for me. Anxieties. Worries. Difficulty in driving at night. Extremely bad, almost unbearable. Panic.	Adrenalin taken the same. I believe that it is around this time that I must have been using different adrenalin. As I recollect, the clean adrenalin still had some colouring left from the inside of my bottle. Whether it was the same colour as originally, I do not know, inasmuch as small parts of tobacco sometimes get into my nebulizer. However I think I alternated between the two. (The other bottle was Epinephrine Solution 1:100, Burroughs Wellcome & Co.)
From Wednesday, 27 June to Wednesday, 4 July	Compulsive thoughts. Visual stimuli the same. Anxiety and depression.	Found it hard to concentrate on other thinking which I normally would have occupied myself with. My whole mental activity was at a qualitatively different nature than it had been prior to the initial taking of the adrenalin.
Tuesday, 10 July Trip to Newhaven	Anxiety was bad. Compulsive thinking.	However, I was looking forward to discussing my situation and felt that if it was personal anxiety that I would find out. I found that I did not need to use the adrenalin; something which I had felt for some time. I decided not to use it. Probably used it only four times on the trip.
From Tuesday, 10 July to Friday, 13 July	The anxiety was not as bad. Depression not as bad.	I spent my time fully discussing every aspect with a very close friend.
Friday, 13 July	Improved, but still in the background.	I had a discussion where adrenalin was first mentioned as a possible cause. <i>I stopped taking adrenalin.</i>
Saturday, 14 July	Greatly improved.	The thought that it could be adrenalin seemed to aid me greatly.
Sunday, 15 July	Much better. Only occasional thoughts.	
Returned from Worcester.		
Monday, 16 July	Again great improvement.	Read Gerard's article.
From Monday, 16 July to Saturday, 4 August	Varying, but sometimes feeling worse and some days feeling almost normal.	Would feel worse when I lost insight and thought perhaps it was not the adrenalin.

Time	Symptoms	Comments
Saturday, 4 August	In the afternoon picked up book. Fair in my feeling of strangeness.	Read article by Hoffer and Osmond. Tremendous qualitative difference in feeling. Felt a sustained recovery (I felt that this article was specific in that I could test my feelings against others, where Gerard's was so general). Once the feeling of sureness, every day seemed better than the last. What helped most was when I found that the drug <i>should</i> wear off and not be retained. This thought helped me see an end to the ordeal.
Friday, 24 August	Went fishing. Felt good. Broke a bone in my foot and ruptured a blood vessel. Still felt good but felt depressed when thinking about my past experience. A general feeling of unease. (This was occurring less and less.)	
Month of September	Through the month of September only thought about the strangeness of the episode and therefore still looked for visual distortions rarely, but would notice none. Returned to normal physiological and psychological feelings.	
Monday, 17 September	Short of breath with the change of weather to cold. (Something that seems to happen every year.) Have again taken adrenalin every night and sometimes in the morning. Feel entirely normal except an occasional worry as to whether the whole episode showed some imbalance in myself, and at present, am very productive, physiologically normal, psychologically alert and have been able to do a great deal of planning and feel myself as I used to be. The tendency to dispersonalization seems to be entirely reversed as I feel as I used to.	

GENERAL COMMENTS

1. All through the time from start to present (actually the situation has been on for $1\frac{1}{2}$ years, sometimes much, much worse without any feeling of the kind noted or anxiety) I have been in a personally anxiety-producing state. However, now that I am back to normal, it does not bother me, and it did not before I took the Epinephrine (discoloured).

2. My physiological feelings can be described (when I stopped taking the Epinephrine) as insomnia—not being sleepy, not falling asleep at times for a whole night, then trembling especially of the lower lip when I laid down, as if I was too “charged up”, uncertain visual patterns and shapes (few times) right before I dropped off to sleep (never experienced before or since); feeling aroused and much nervous energy. No asthma or wheezing at night under any circumstances (except once when I was at the home of a friend who had dogs), completely without the symptoms (asthmatic), even after heavy exercises.

It can be summed up by saying I felt like one is supposed to feel when they have had a lot of adrenalin.

3. I experienced several anxiety-producing situations; one in a talk and a drive to another city (21 August). I looked with dread on both (normally I would have been looking forward to it). I felt that driving for several hours and alone with my thoughts would re-activate my anxiety and fear and compulsive thinking. It did not (that gave me self-confidence). Another time when I went to a doctor for a urine analysis for an insurance policy, I was called and told to come back in two days as there was something wrong. While anxious for a

while, it was a normal anxiety and did not cause me any recurrence (14 August, 1956).

Personal data: Born 7 September, 1922; married; two children.

His friend writes, "In answer to your questions, I did look at the discoloured adrenalin but I don't actually remember what it looked like. I feel the colour I name is almost a guess. I do vaguely remember it to be a sort of colour of rose wine, a light yellowish rose. He did mention its being discoloured but took it nevertheless. At first I didn't feel his upsetness was more than I would have expected under the circumstances, but this may have been because he didn't tell me how bad he felt so as not to worry me. He said that when he was with me he felt good and like himself. I made him feel normal again, and it was when we were apart that he had the strange thoughts, fears etc. His feelings as he described to me were very strange for him, not at all like him. He told me details of his extreme feelings of anxiety, strange visual perceptions etc. He was looking for all sorts of psychological causes for these feelings rather than looking more objectively for physiological explanations as I would have expected him to do. I kept trying to find a cause for his extreme anxiety which he felt was out of line, even though he had expected to be upset by my going, he hadn't expected it to throw him that much. I asked him if he could think of anything unusual, any drug that he had taken, etc., which might account for this and he suddenly remembered the discoloured adrenalin but was loathe to lay the cause to it. I remembered Gerard's article, 'The Biological Roots of Psychiatry', in *Science* of August, 1955, in which he spoke of adrenalin metabolic products as being possible causes of mental disturbance. He went back to Chicago feeling immensely relieved that the adrenalin might be the cause of his troubles. He had always been so sure of himself, so proud of his way of thinking and so unneurotic, so having these thoughts came as a terrific shock to him. Yet he kept saying he didn't want to lay all the blame on the adrenalin but I was sure that was it. He never showed any strange symptoms, had any thoughts or feelings before the adrenalin as he did in the period after taking it.

"I felt the episode was very unlike him and from his report he is entirely better now. When I last met him he was (at the end of January, 1957) completely his old self, but that's not surprising. He always acted and felt okay when with me. He used occasionally to fear the possible recurrence of anxiety thoughts but didn't get them. What I think is most amazing is he doesn't ever need to take adrenalin any more. No more asthma."

This lady's report, like that of his wife and brother and his own account, bears out our impression that this was an exceptionally disturbing event and one that was unique for Mr. Kovish.

Part of Dictaphone Transcription of Meeting with Mr. Kovish in New York on 6 November, 1956

Osmond—I wonder perhaps now if you would tell us a little about the unusual disturbances in perception you had, particularly those dealing with seeing groups of people. I think those were extremely interesting and we would like to hear a little more about them.

Kovish—I remember groups of people bothering me. I felt that there was something distorted or different appearing about them. This was not localized to any one person. There was a strangeness about seeing groups of people which had a somewhat frightening effect on me. I noticed distortion in many things, some animate and some inanimate. As far as the animate subjects went, it did not seem to me that women looked strange but it seemed to me that the

faces of the men particularly had a different sort of appearance than I had ever seemed to have noticed before. I can't really explain it, except to me, it seemed sort of distorted. In order to try to find an explanation to myself for this, I recalled a conversation of just a little while previous to this happening with some psychologist friends of mine about a review I had read in which it had been stated that one of the tests for latent homosexuality according to one of the writers was the fact that the recognition of an object of the same sex means latent homosexuality. I am heterosexual; it did not affect my sex life. I did not feel that I was a latent homosexual or a homosexual or anything. However, this thing became very obsessive to me. I thought that the possibility of what if I was and it seemed to me that it was the only reasonable explanation that I had heard as scientifically minded people had written something about this—that if people appeared distorted to me, especially men, that maybe I was. However, the peculiar part of this, and it bothered me, at no time did I believe I was, at no time did I feel that I was or were there any reactions of that nature to suggest that I did and the fact that I couldn't shake off something which to me was so ridiculous to my mind, so untenable, frightened me considerably.

Hoffer—One question Mr. Kovish regarding your vision. How long have you been wearing glasses, and during this period were you especially sensitive regarding your eyes or the eyes of other people and did you notice any type of visual disturbance that you have not had before or since?

Kovish—I thought quite a bit of it at the time. I have been wearing glasses since I was seventeen years old and did not associate any visual problem with it, except that I wore my glasses, sun glasses, more than I had, I seemed to be sensitive to light and I felt the direct relationship between my visual distortions, when I say visual distortions, it is hard to describe. I saw people more or less like I have seen before except that they impressed me differently, and this was very frightening to me and what happened was that I panicked, I mean, I could find no logical reason for this strange thing that was happening to me and I spiralled downwards to the depth of despair at having lost my mind and I felt that that was a very disgraceful and shameful thing to do.

Osmond—May I add at this point. You say that it seemed as if you had seen these people before. I wonder if you could pick that point up and the other point in which you say inanimate objects also seemed to have changed. Could you give us some examples as exact as possible.

Kovish—When I say I saw these people before, I meant that they were people who were very familiar, who I had seen all the time and never thought a thing about. I had seen them all my life, some of them. There were no distortions previous. All of a sudden, they appeared strange to me. There wasn't a strangeness, such as two heads and four arms; it seemed to be some strangeness that was inherent in what I was seeing and yet nothing that was recognizable as to being that different to me in their appearance and this is what bothered me. All of a sudden if I saw horns growing out of their heads, I don't think I would have been nearly as worried. I would have known that I had gone stark raving mad but the fact was that there was something that I couldn't quite fathom happening to my visual perception. This was greatly noticed by me at the time when I was worst off where I went to a movie. Movies to me have been sort of an escape and instead, it seemed the figures on the screen, well put me in a state of anxiety and despair and certainly, there was nothing relaxing. One that I have just recently come out of, that is I can go to movies and think, how can I ever feel that way; it still has a triggering aspect of making me think of how I felt

then even though I feel or see nothing different. As far as inanimate objects, it was a certain change which can hardly be described. It was an internal change. I mean, this girl and I were riding in a car and I told her that—and she repeated this to me later, I can remember her saying it but it was good that she witnessed it, because I didn't know of any way I'd expressed it before—I said that it seems like the trees are foreboding, it seems like the road is threatening to me. I mean I described to her what seemed to be a strange feeling and yet there isn't any real change in my vision except what I look at seems to disturb me internally and that is about the best way I can describe how inanimate objects affect me.

Osmond—When you were driving the automobile or being driven in it, did you notice any difficulties in perception then in particular regarding time and distance?

Kovish—Yes. Again, it wasn't anything that I could rationalize. I noticed, for example, one evening after I first took the adrenalin, coming back at night, I had driven the road many times. It was about four o'clock in the morning. I usually fall asleep quite often on the road, barely avoiding getting killed. Well, this night after having driven several hundreds of miles during the day and having had no sleep at all over the weekend to amount to anything, in driving the road looked different to me. It was a very familiar road. I was alert to an extreme that I had not been alert previously and I recalled going off the road which I had done in the past only under the influence of being sleepy but not on the influence of being awake. I have done much driving, sometimes on test runs and considered myself a very skilful driver. This seemed to me extremely strange.

Osmond—When you say that you saw familiar people looking strange, how about strange people, the figures you saw in the film.

Kovish—I think I should say that I was able to reconcile myself to the familiar people very easily, by saying—now look, you have seen these people before. They are not really changed. They haven't changed. It's got to be you and I wouldn't think about them being strange; so it was only the passing time when I first see them. Everybody else passing on the street, everybody would institute these thoughts in my head that there was something different about them. I felt in some instances—I remember this was the strangest thing—I was getting a haircut and I remember I have seen the barber before but I never recall having a haircut by him but it seemed to me that he seemed quite different being as close to me as he was, quite strange and I felt, for instance, I don't know as I would say threatened but I didn't feel comfortable. This is as close as I can get. I really can't. I really don't want to answer that because I don't recollect anything except feelings of foreboding which I felt were generalized and not specific to any one person. However, I am sure that there was some feeling of that, it couldn't have been that strong for an individual person. I think it was more—, for example, I would stand in a crowd, like at an airport, like Washington airport; I used to enjoy standing in airports and watching all the strange people going by. I mean everybody looked different; it was a pleasure observing people going by. However, the very hurry of the people going by disturbed me greatly and initiated these compulsive thoughts in my mind—why am I looking at them and so on. Actually standing in an airport was—became a hideous torture to me rather than any pleasurable thing as it had always been in the past.

Osmond—How about relationships with those close to you—your own wife and children and your close relations, like your brother and people of that sort. Were these either enhanced or altered or did they remain just the same?

Kovish—I would say, that it is my point of view, knowing that there was something wrong, I tried to be understanding in my relationships with other people but I was extremely irritable, felt low, couldn't participate in the normal social functions of my family that I always had, such as playing with the children properly and discussing things with my wife and things of that sort. I would like to go back to this physical, the inanimate objects again. I just happened to recollect that I did feel extremely—in flying, in other things which I normally take for granted, that I do quite a bit of, or even driving—I did feel a sense of foreboding and threatening. I don't think I can put it in any more general form, except that I became almost terror stricken at times for no apparent reason.

Osmond—When you were driving, were you certain where the other vehicles were?

Kovish—I feel, I feel that there was some impairment in that, but it was of such a nature that it was only relative to previous feelings that I had nothing really to measure it by. I did feel that, for example, lights of a car would come, all of a sudden, it was just as if I had gotten a shock reaction. I felt that it was going to hit me or something of that sort. That is the only thing that I can recollect.

Hoffer—Were you aware of any movement in your visual field either around the edges or in the centre, any type of slight quivery movement.

Kovish—I don't believe so.

Osmond—I wonder if you would perhaps put on this record the bit you told us about the family and the history. It needn't be too long.

Kovish—I have given considerable thought as to my family history and was unable to find anybody who had either required any kind of doctor's care for mental illness or any kind of upsets or breakdowns of any sort that seemed to me—. The only thing that I found was that my brother at approximately I would say the age of fourteen, I don't know exactly, he is twenty-eight, I think had some strange sensations which were diagnosed as epilepsy. I wasn't living at home at the time. I had never seen them. I guess they were rather rare. I understand they don't recur but it has never been discussed. He doesn't like to discuss it. I actually lived and worked with him for over ten years and have never noticed this in him. He is also asthmatic and I have found, as far as I can see, I think he must have had some sort of minor epileptic episode at one time in his life. I know nothing more about it. Mentally, he cannot be classified as either neurotic or psychotic or having any tendencies that I can see and I have known him very closely.

HISTORY AND DESCRIPTION OF MR. KOVISH

He is a personable man in his middle thirties, about 5 feet 8 inches. His body build is lean and muscular, though by no means lacking in a fat component (Sheldon would describe him as being a mesomorphic with ectomorphic features, though by no means endopenic). He moves gracefully and is neat and well turned out, though not dandified. His co-ordination is excellent. He drives a car with skill and aplomb. His hair is dark and plenty of it, his features aquiline, his skin clear and without blemishes, his eyes brown. His beard growth though strong is not excessive.

His manner is friendly and open. He can state his opinion and hold his own in discussion on a variety of subjects without being either subservient or overbearing. He has a lively sense of humour, and although of a curious and enquiring nature, is tactful, so that he is not in the least impertinent.

He was born in New York of poor Jewish parents. His father worked as a garbage collector and secondhand clothes dealer. His mother, who is still alive, managed to keep the family going in spite of many difficulties. He did well at school but left early to earn. At 14 he began as a helper in a small construction firm. He soon acquired extra skills by attending night school, correspondence courses, etc. and achieved a high degree of mastery in his work. He became recognized as an expert in developments in structural engineering so advanced that they are only just finding a place in formal engineering education. Patents which he made on some inventions have allowed him a great amount of independence. His work in recent years is largely of a consulting nature.

He was always keen on Trades Union work and this interest has not been reduced by his new status of a semi-managerial sort. He is a radical, anti-racketeer and anti-communist. He is very critical of both political parties and votes for neither, but he is not indifferent to politics. He is sharply aware of the shortcomings of the society in which he lives and does what he can to encourage those with whom he comes in contact to think. He has not however become either a bore or a fanatic on this account. To further his knowledge he has an honorary position in a social science department of a local university where he puts his knowledge of the construction industry at the disposal of the scientists.

Apart from asthma his health has been excellent and although this sometimes troubled him gravely, it has never prevented him from leading a full life. He has taken adrenalin by inhalation regularly for at least a decade and has never had any disturbance from it previous to the one we report here apart from a light headache when he took some that had turned black. Changes in weather he considered affected his asthma much more than fluctuations in his emotional life.

His family have been healthy. He knows of no one who suffered from serious mental illness. His younger brother has asthma and hay fever and may have had minor epilepsy in childhood.

Mr. Kovish does not consider himself nervous nor does his wife, his brother or a friend. Rather the reverse—they say he is a man with good reserves of humour and optimism even when things seem difficult. Consequently they were very much surprised and disturbed when he seemed so unlike himself in mid-1956. They had none of them seen him like this before. His brother says he will sometimes withdraw from a party or conversation, but this is only when he is preoccupied with new ideas in his work. Mr. Kovish prides himself on his ability to cope with social, professional, business and domestic problems. It was very distressing for him to be unsure whether he could do this any more.

He holds that he is sexually well adjusted. In his subculture, sex mores were free with little guilt and no recrimination. He has never been much pre-occupied with sex as a problem because it was not considered to be one. He married twelve years ago, has two children. In the last three years he has been in love with a married lady. For various reasons they both recognize that nothing can be done about this. His wife and he have not quarrelled over this, they had discussed the matter freely so that it has been less a source of tension than of puzzlement as to what was the right thing to do. When Mr. Kovish took the wine-coloured adrenalin solution this situation had been in existence for at least eighteen months, and far from becoming more difficult he considers that he was getting accustomed to it. Shortly after he first took the wine-coloured adrenalin his friend broke her arm and also left town, although this was long expected and prior to it had not been looked on as a source of worry. He ascribed his upset to this.

OUT-OF-DATE ADRENALIN FOR INHALATION

Mr. Kovish remarked on the unusual colour of the adrenalin solution which he bought on the evening of 20 May. It seemed odd to us if asthmatics frequently take a wine-coloured solution that more cases of this sort have not been reported. We were assured by the Connaught Laboratories and the Parke, Davis Company of Canada that they have not heard of any. While this might be due to failure to see any connection between discoloured adrenalin and the development of psychotic-like conditions, another possibility is that commercial adrenalin very rarely goes this colour. Thanks to the courtesy of these two manufacturers, we have now examined over 500 bottles of discoloured 1 per cent. adrenalin solution used for inhalation. This solution was examined by eye and then samples of the discoloured material were examined spectroscopically.

Mr. R. Hall, R.P.N., S.P.N.A., examined the solutions and used this procedure. Any solution that was pink, red or yellow was preserved. Brown solutions, of which there were many, were discarded except for a sample which was then examined spectroscopically by one of us. These solutions which range from cloudy brownish to some having a thick black precipitate, gave no evidence of adrenolutin or adrenochrome. There were no pink or yellow solutions.

It seems that adrenalin solutions used for inhalation turn wine-coloured very rarely though it might be a wise precaution to advise asthmatic people very strongly against using any discoloured adrenalin, but particularly that with a pinkish, reddish or bright yellow hue.

OUR EXPERIMENTS

We are reporting two incomplete series of experiments because they illustrate some of the difficulties inherent in research of this sort. These experiments are inconclusive but we think they are interesting and suggestive. Their design is, of course, unsatisfactory, a common feature of early work. With Mr. Kovish's experience, they may be worth following up.

A.H.'s Experiments

These were begun as an exploration after we had heard of the effect of inhaled discoloured adrenalin on Mr. Kovish. Later we started a double blind design but discontinued it after we found that our adrenolutin had deteriorated. Using resin absorption columns, we have estimated that only 25 per cent. of our adrenolutin was the pure substance. The rest of it had become a variety of melanin-like compounds. We have included details of two out of five of these experiments, all of which showed in our view significant, though small changes. Minor changes of this sort are hard to describe accurately.

FIRST EXPERIMENT—INHALED ADRENOLUTIN

ADRENOLUTIN EXPERIMENT—A. HOFFER

20 September, 1956

- 2.10 p.m. Inhaled $\frac{1}{2}$ mg. of adrenolutin in phosphate buffer at pH 7.25.
- 2.18 p.m. An additional $\frac{1}{2}$ mg. in buffer.
- 2.15 p.m. Slight suggestion of tension in the temples similar to onset of headache. No change in pulse rate.
- 2.25 p.m. No noticeable change.
- 2.26 p.m. Slight moistening of palms of my hands and heavy sensation in temples.

- 2.27 p.m. Slight feeling of unsteadiness while working, still a suggestion of headache, speech feels a bit thick, and no change in pulse.
- 2.58 p.m. Slight feeling of unsteadiness since last report. Spent time with Dr. — discussing some somato-typing. Found this dull. He appeared very distant from me. Feel less secure with myself. Palms are still sweaty. Headache definitely present but a bit higher on temples. Some difficulty in focusing vision.
- 3.20 p.m. Headache is gone; feel more secure. Spent some time with Dr. —. Feel not in control of the situation.
- 3.55 p.m. Feel quite normal now.
- 4.00 p.m. 1 mg. adrenolutin taken—same preparation as above. It is now appreciably darker than it was.
- 4.24 p.m. Feel tired, disinterested and sweaty.
- 4.25 p.m. Sweating palms.
- 4.29 p.m. Still sweaty and feel warm. At this time, the room was warm and Miss — opened the window without noticing that I felt warm.
- 4.45 p.m. Feel O.K.

FOURTH ADRENOLUTIN EXPERIMENT (INHALED)

5 October, 1956

I inhaled 20 sprays, approximately 1 ml. at 11.00 a.m. of the usual adrenolutin preparation in saline and phosphate. About two minutes afterwards, I noticed the same sensation of tension in my forehead, a slight sweating and a continual feeling of tension around the forehead and eyes for about twenty minutes.

About 11.30, I was interrupted by a visit from some senior members of our department who strongly supported the research programme and advised me that we would not be short of funds. This produced in me a strong feeling of euphoria which continued throughout the day.

At noon, I noticed no effects from the drug and concluded that I had had placebo. I had a normal lunch but was over-talkative at the luncheon table. We had just received an extension ladder which I had ordered and as I had not yet put on my storm windows, I decided to try out the ladder. As the coffee was not quite ready, I went out and put up a couple of windows in between the main course and my dessert. I noticed while putting up the windows some unsteadiness which I usually do not have and at one time while I was on top of the ladder with a window, I had the feeling that the bottom of the ladder was going away from the house. This was only momentary. I managed to put up a couple of windows and then finished my dessert. Looking back on this behaviour, it appears that my judgment was a bit off. I had had adrenolutin and this illustrates the importance of not making a prediction while under the possible influence of drug.

H.O.'s ACCOUNT OF HIS EXPERIMENTS

One way of following up the lead that Mr. Kovish had given us was to inhale decomposed adrenalin as he had done and see what happened. Our original intention was to try a commercial sample of the sort that Mr. Kovish had used but this proved so resistant to oxidation, remaining gin-clear though boiled for several hours in a test tube, that instead a home-made solution was used.

METHOD OF PREPARATION

Fifty mg. of adrenalin supplied by Dr. Hoffer was weighed and placed in a 15 ml. test tube. Five ml. of normal saline was added. Only a little of the adrenalin dissolved, most lay in the bottom of the tube. The supernatant fluid slowly went a pinkish colour and to speed this up it was placed in hot water. When the adrenalin failed to dissolve when put in hot water, two to three drops of dilute hydrochloric acid were added. The residual adrenalin then disappeared and the solution became a very pale pink. This solution was then placed in hot, not boiling water for perhaps one to one and a half hours and became a clear salmon pink or "vin-rosé" colour. Another tube started at the same time but heated longer before the acid was added became a pale sherry colour. This was done on the evening of 16 October, 1956 and the two tubes spent the night in the deep freeze of the refrigerator. Next morning (17 October, 1956) the sherry coloured tube (yellow) had frozen solid and on unfreezing it seemed darker than before with some brownish pigment showing. The vin rose-coloured tube was not frozen solid but lying in the pinkish liquid were some long, lanceolate crystals which were transparent and white. These disappeared on warming. The two tubes remained on ice outside the deep freeze but did not freeze again. Their colour remained constant to the eye and stayed so for several weeks when kept in the refrigerator.

THE FIRST EXPERIMENT

To make this account more understandable, a few domestic details may help. My daughter was ill with 'flu on the evening of the experiment and was sleeping in our bedroom with my wife. I had not told my wife of the projected experiment because a minor one a few days previously had been negative and I expected this one to be so too. I was confident that nothing much would happen. I had a notebook on hand and prepared to make continuous notes. I did not have a recorder. I used a deVilbiss 33 sprayer which throws a fine cloud of small particles which can be inhaled easily. It takes about three minutes of vigorous pumping to get 1 c.c. into the mouth. I put a little more than 1 c.c. into the sprayer which would mean about 10 mg. or so of adrenalin and its derivatives. My concern was that I might be made uncomfortable by the residual adrenalin. However, I know that I am sensitive to it and respond quickly. So placing the sprayer in my mouth, I made a few squirts and inhaled deeply and watched carefully for sweating, palpitation, dryness of the mouth, or any change in pulse. None came. My resting pulse is 64 to 70 and during the experiment it remained steadily at about 66.

The first inhalation was at midnight 18 October, 1956 and the notes read as follows:

At 00.05: "I noticed things seemed very clear. A piggy bank of Helen's (my daughter) held my eye quite a time. No palpitations, sweating, tremors or dry mouth." By 00.15 hours I had had three good bouts of puffing. I felt there was some increased significance of objects and a raised general awareness. I looked in the mirror and noted that my pupils were not enlarged, my hand was steady. 00.22—Fifth puffing. "No very certain feeling except a slightly warm feeling in the face and a poised feeling, not unpleasant, of everything being sharp and significant."* 00.30—"Slight feeling of nausea, not very much, running nose. An undoubted sharpening of perception. Looking at my watch

* After reading the first draft of this article Mr. Kovish wrote to us "I could not have explained my first night reaction in any better terms, except to add the term colorful."

about three feet away, it seemed an immeasurable distance. Everything I look at holds the eye. The sinking feeling in the stomach definitely there. Mouth moist, slightly anaesthetic feeling in the roof of the mouth. Discomfort in the pit of the stomach. Pulse 64. Some bowel discomfort and farting." 00.35—"Marked bowel discomfort—sinking feeling in the pit of the stomach, not exactly nauseous, unpleasant. This may come from having swallowed the stuff." 00.45—"Defaecating, difficulty in writing because of sinking feeling. Stool well formed. Feel bad." 00.50—"Weak, solar plexus kicked feeling. Very uncomfortable, cannot write feel so ghastly." 01.00—"Very weak and uncomfortable. Kicked in the stomach feeling—objects hold me. Weak, very uncomfortable, alone, lonely, beyond feeling, cut off. Know this is very interesting, don't feel so. Very fatigued but wakeful, alert, uncomfortable and disinterested. Beastly. No headache. Can think clearly but no feeling. Can write but feel too weak to move." 01.10—"Another bowel movement. Suddenly began to feel much better. Hope it continues." 01.20—"Cleaned my teeth which I felt incapable of doing before and came back to bed. Still get discomfort but not so bad as before. Comes in waves. Not nauseating but very unpleasant. Only visual signs increased significance of objects." (No notes for the next three hours. My writing fades out on the pad at this point. I lay on my bed awake but did not like the bed clothes touching me. Objects had a curious significance; leaves on a tree outside might have been a snake. The size of the room seemed uncertain. I had great difficulty in initiating action. But once I managed to do this I could act. For instance, I let our dog out of my room. I was concerned that I might get worse and die—in a very detached way. I was perfectly well orientated and knew where I was. I knew that I wanted to write but I couldn't summon up the energy to do so. I seriously considered trying to summon a colleague or my wife, but felt that she would be worried and that the colleague, in his concern for me, might give me some morphine and that this might well kill me. I decided I was safer as I was.)

04.30—(notes continued). "Vomited. Incapable of writing in the interval. Awake but lessening discomfort. Curare-like effect, some slight visual disturbance, but most acute aspects have gone now.

(i) Either this stuff has a mule kick, or

(ii) I very suddenly developed flu at 00.00. I felt very well then and in no way expected anything of the sort."

10.45—Awoke about 07.45 feeling very weak but noticed the perceptual changes had gone. No nausea or vomiting or bowel discomfort but considerable lethargy. However, got up and felt much better. Have been at work. Don't look as if I'd been ill. No fever or dry mouth. Breath clean.

15.10—Did my rounds, attended conferences, made decisions as before. I do not even feel very tired.

20 October, 1956—I did a further experiment using the same vin rose-coloured substance which had been kept unchanged in the refrigerator. It remained in this condition for several weeks after, because I'd hoped we'd be able to analyse it but at the time we were not able to do so. The question was whether it was the fraction that had been atomized and inhaled which had been effective or the fraction which had been swallowed. I put 1 c.c. into a wine glass and noticed "odd etheric smell which is difficult to account for", taste "slightly burning". (Note: Although I had put a little more than 1 c.c. into the spray on 18 October, 1956, next morning there was still about half of this left so that at that time I must have got between 5 and 7 mg. of adrenalin by-products. This time I swallowed 10 mg. of adrenalin by-products.)

23.35—I wrote “the 1 c.c. must have 10 mg. of adrenalin by-products in it. I am somewhat apprehensive after my previous experience but this seems a necessary course of action. However I am hopeful that it will have no effect by this route. ‘Little H.’ (my daughter) making a beastly row, having woken howling. The cries are irritating and I am edgier than last time. Pulse 64.”

23.45—“H. still bellowing. I feel irritable but this is probably not abnormal. Warm feeling in my substernal region unpleasantly reminiscent of the last episode.” 23.50—“No heightening of perception yet. Some slight gastric discomfort—heaviness, apprehension.”

21 October, 1956. 00.05—“Quite marked discomfort, heaving feeling in the epigastrium—no perceptual changes. Though do feel very alert. I hope this damned thing is not starting all over again.” 00.10—“Gut rumbling, belching, not very much, and farting (once). Pulse 67. No perceptual changes that I can notice at present. Am reading Slotkin’s book with enjoyment though some concern about another night up and hope I am mistaken. What I’d not considered was that this stuff might become more potent on standing in the refrigerator.”

00.25—“My impression is that the discomfort is less than last time. I have not yet had a bowel movement.” 00.30—“Went to toilet but didn’t have a bowel movement.” 00.45—“Some discomfort, no lethargy, apathy or lack of energy. Reading Slotkin’s book with keen enjoyment and marking relevant paragraphs. Keep wondering if this is just suggestibility, but don’t think so. I am not commonly suggestible. It just seems a very minor variation on Thursday’s massive theme.” 00.55—“Unless something unexpected happens this is now passing over, a little belching and no more. Merely the ghost of that other night when at one time I thought I might die.” 01.10—“Still some gut uncertainty but not very much. No weakness etc., I am going to sleep. Either the stuff’s potency has fallen or it is less effective by the stomach than by the inhaled route.” 09.00—“Awoke feeling well, if slightly tired—not irritable.” I did a day’s work without feeling any difficulty and had no residual troubles.

DISCUSSION

1. *Mr. Kovish’s Experience*

Those who work with psychotomimetic agents and who come to believe that they may serve as models of naturally occurring psychoses, find that much is made of the difference between the laboratory experiments and the illnesses being studied (usually schizophrenia). This criticism, so far as it goes, is valid. One might expect the critics would hasten to fill this gap in our knowledge by suitably designed experiments of their own. They have not done so, even though they repeatedly emphasize how slender a resemblance the highly artificial psychosocial setting of the laboratory can bear to real life; while the most refined double blind experiments usually leave the subject well aware that something is happening.

Why then do critics carp without advising, because they must know well enough the sort of experiment necessary to prove or disprove this point? It is simple and entails giving repeated doses of a particular psychotomimetic agent to an unsuspecting and unprepared subject without the knowledge of his family, friends or those with whom he works, while closely observing him for a more or less prolonged period. However scientifically desirable this might be, it is ethically impossible and would never occur unless nature took a hand.

Thanks to Mr. Kovish's permission and nature's co-operation, we have the nearest thing to the sort of experiment which it would be improper to essay.

What then happened to Mr. Kovish? A short time after oral inhalation of small quantities of a wine-coloured solution of 1:100 adrenalin hydrochloride he noticed some changes in the brightness of the room where he was. One of the persons has reported a rather similar experience after taking adrenochrome (15). While accounts of unusual light are not infrequent with schizophrenic people—for instance Madame Sechehaye's Renée (33) refers to it in this manner: "... I ran home to our garden and began to play 'to make things seem as they usually were', that is, to return to reality. It was the first appearance of those elements which were always present in later sensations of unreality: illimitable vastness, brilliant light and the gloss and smoothness of material things. I have no explanation of what happened or why."

Later in that same night while driving, although he felt alert and wakeful, he twice went off the road. We have reported a rather similar experience after adrenochrome. At first Mr. Kovish did not connect this with the discoloured adrenalin which he continued to inhale, as was his wont. He became aware of changes in perception of a subtle sort, especially when driving and when he saw groups of people. Because of previous discussion with a psychologist friend he wondered whether this might mean that he was a latent homosexual. He became unshakably pre-occupied with this disturbing thought. At the end of about a week of increasing discomfort and anxiety, he wondered whether the discoloured adrenalin could have anything to do with his predicament, but he dismissed this idea as a rationalization. His symptoms continued and his disturbances in perception and thinking were accompanied by anxiety and depression. These were noticed by his wife and brother who ascribed them to pre-existing domestic difficulties. He ruminated increasingly on the strange appearance of people. Here again this is not unlike Madame Sechehaye's Renée, "One day we were jumping rope at recess. Two little girls were turning a long rope while two others jumped in from either side to meet and cross over. When it came my turn I saw my partner jump towards me where we were to meet and cross over. I was seized with panic; I did not recognize her. Though I saw her as she was, still it was not she. She seemed to be smaller but the nearer we approached to each other the taller she grew, the more she swelled in size." There are many references in this excellent book to perceptual changes of this sort. Because of this rumination, his usual pre-occupation with his work, which is of a creative sort, was reduced. However, when he did get down to work he believes that its quality was if anything enhanced and he has evidence of this in some of the ideas and projects which came to him during that time. It is of course, hard to prove this but Parfitt (29) and Fitzherbert (11) both suggest that in some phases of schizophrenia, at the beginning of the illness in particular, intellectual performance may be increased. At the end of about four or five weeks in which he took the discoloured adrenalin, he diluted it with clear solution from another bottle.

When his friend suggested that the discoloured adrenalin might be responsible for his condition he stopped all adrenalin on Friday, 13 July. But it was not until 4 August, after reading the literature that he became convinced and his apprehension lifted. After this he improved rapidly. Some of the symptoms which he describes can be found in such famous accounts of schizophrenic illness as those of Boisen (5), Beers (4), Hennel (12), Ogdon (26), Judge Schreber (31) and Madame Sechehaye's Renée, already referred to, particularly in the early phases of their illnesses. They have also been described in experiments

with psychotomimetics such as mescaline, LSD-25, adrenochrome, adrenolutin, ololiuqui, ayaheusca, and hashish.

Again, it is particularly in the early phases of experience with these substances that one gets these "mild" but persistent disturbances. Mildness after all is a matter of one's viewpoint and Mr. Kovish himself has emphasized how much easier he would have felt had he seen people with two heads or with horns coming out of their foreheads. Then he'd have known for certain there was something wrong. As it was he was left in extreme uncertainty.

After our careful investigation of Mr. Kovish's story, we believe that he is telling the truth. We know that he is what he claims to be. But what can we learn from him? How could such a small quantity of decaying adrenalin produce such grave and sustained effects? The 5 c.c. of 1:100 adrenalin for inhalation can at the most contain only 50 mg. of degradation products lying between adrenalin and melanine. Is that enough? Our experiments with adrenochrome and adrenolutin suggested that by vein or by mouth doses of at least 20 mg. would be needed to produce these effects, and probably twice as much. How then could 50 mg. by inhalation produce such remarkable changes in him?

There are at least three explanations possible:

(i) Mr. Kovish might be unusually susceptible to adrenalin and its by-products. He has however taken it regularly for over ten years, sometimes in large quantities and has never had an experience of this sort. He seems, like many asthmatics, to be more tolerant of adrenalin than most people.

(ii) There may be a very powerful psychotomimetic agent or combination of agents in wine-coloured adrenalin. While our work with adrenochrome and adrenolutin gives some support to this, the colour described by Mr. Kovish is far too light for the whole 50 mg. to have been converted into adrenochrome or adrenolutin. Indeed it suggests that perhaps only a few hundred gamma and at the most a milligram of adrenalin had been changed to these brilliantly coloured compounds. Here is a hint that at least one other adrenalin derivative may be present whose existence we have suspected but not yet proved. We know that LSD-25 is effective in doses of 100 gamma or less, may it not be that an adrenalin derivative exists which is as powerful as the ergot compound?

(iii) Perhaps inhalation is more effective than other routes of admission. Hashish, opium and henbane (30) are particularly potent when inhaled. Hofmann's (35) original LSD-25 experience arose from accidental inhalation and so did Sherwood's (34) discovery of the psychotomimetic nature of his B.G.I. Cronheim (7) has shown that adrenalin itself is more effective given this way than by other routes.

Oddly enough at first the full implications of the lung route were lost on us because of an error, which readers of the Macy Foundations Transactions of the Second Conference of Neuropharmacology (2) will find, that we committed in company with far more learned men than ourselves. This was to assume that asthmatics commonly take adrenalin through their noses. To test this one of us snuffed up 50 mg. of adrenolutin with no effect except a handkerchief heavily stained with a melanine-like pigment. It is true that some South American Indians do inhale cohoba snuff, said to contain bufotenine, and an immensely powerful snuff derived from a nutmeg-like tree, through their noses but they use a specially designed apparatus for this which ensures that the particles of snuff are driven at high pressure into the lungs. Most asthmatics however, inhale their adrenalin by mouth using some sort of atomizer.

2. *Our Experiments*

A.H.'s experiments suggest that an exploration and comparison not only of adrenolutin but of every other psychotomimetic or psychedelic using the lung and other routes, is now overdue. It must be remembered that the adrenolutin that A.H. used was, owing to difficulties of manufacture, by no means pure. At most about half of the amount which he inhaled reached his lungs and if all of that was quickly absorbed, only about a fifth of that half went directly to the brain. Since we know that this quantity, about one-tenth of a milligram, taken by mouth or by vein, would be wholly ineffective, we are left with the strong hint that when adrenolutin can go directly to the brain by the lung route, it is enormously powerful. We have not yet been able to repeat this work on a large scale because of our continuing difficulties in synthesizing adrenolutin, but we believe that these have lately been solved.

H.O.'s experiments were intended to be part of a series starting with the pale pink substance which he describes, and moving slowly to darker and darker derivatives of adrenalin. Owing to the dramatic nature of the first experiment this orderly progress has been halted. The pale pink liquid cannot have contained very much adrenalin because it is unlikely that someone usually sensitive to this substance could have inhaled at least 3 mg. of it (for we may assume that about half of the 7 mg. was swallowed) in so short a space of time without noticing any pressor effects. Yet not very much adrenochrome was present for the salmon pink solution was far too pale to harbour much. It seems likely that there is some soluble, colourless, rose-tinted stable substance without pressor effects lying between adrenalin and adrenochrome. This substance may itself have psychotomimetic properties or may develop them in the course of its journey from the mouth to those brain centres which it seems to affect.

Adrenalin shares with serotonin, lysergic acid diethylamide and adrenochrome the ability to interfere with synaptic transmission of stimuli, Marrazzi (24). When administered into the ventricles of animals it produces changes in consciousness, Leimdorfer (21), and when given intramuscularly to humans, produces tension and anxiety (13) as well as an increase in blood pressure. Because it is toxic and strongly pressor, one cannot determine directly whether or not adrenalin is psychotomimetic. However, the molecular structure of adrenalin may be changed in such a way as to remove its pressor effect. In this event one might have a compound with interesting psychological properties.

The pressor activity of adrenalin may be removed by fusing the side chain with the benzene ring to form an indole; i.e. adrenochrome or adrenolutin or some similar substance or by adding another chemical radical to the free phenolic hydroxyls; e.g. methyl groups. Both adrenochrome and adrenolutin induce experimental changes in behaviour in cats (32), and in humans (14, 15). Adrenochrome changes the behaviour of the Siamese Fighting Fish (1).

When the phenolic hydroxyls are bound by methoxy groups, pressor activity is lost but whether or not these compounds are psychologically active is unknown. Mescaline, which of all the known hallucinogens resembles adrenalin most closely, has no pressor effect. 3·4-dimethyl-phenyl-ethyl-amine, more similar in structure to adrenaline than mescaline, produces experimental catatonia in cats, De Jong (8). It is reasonable to assume that an adrenalin compound with both phenolic hydroxyls bound might have interesting psychological properties.

Tryptamine, like adrenalin, is toxic and a potent pressor substance. In animals it induces experimental catatonia, De Jong (8), in dosages equivalent

in a man to 2 grams. However, about 50 mg. of dimethyl or diethyl tryptamine (Szara, 36) administered intramuscularly produces an experience very similar to that induced by LSD or mescaline, with no change in blood pressure.

It is thus possible that the detoxification of adrenalin *in vivo* under the influence of adrenochrome may result in the formation of a product having strong hallucinogenic properties.

But how does the lung route come to be so much more effective than the gastric or the intravenous? The former encounters the double barriers of stomach lining and liver before it reaches the general circulation and so the brain. But substances taken by vein do not go to the liver but travel only about 18 inches to the heart, and then to the lungs, so that about one-fifth of the amount injected should reach the brain. It is noteworthy that neither LSD-25 nor mescaline seem much more efficient when given by vein than given by the stomach, which suggests that the liver may not have very much immediate effect on either of them. LSD-25 inhaled does not seem to be very much more effective than when swallowed. What could happen in those eighteen inches or so to reduce the effectiveness of adrenolutin so greatly? We know that adrenalin (3) readily attaches itself to red cells and possibly the same things happen with adrenochrome and adrenolutin. This could account for the prolonged action of adrenolutin (15) when given either by vein or by mouth. Perhaps Mr. Kovish's unpleasant experience continued for some weeks after he had stopped taking adrenalin in any form because some of it was released from red cells when they were destroyed. This may be of some importance in a model for schizophrenia. If a naturally occurring psychotomimetic agent was stored in the red cells and possibly elsewhere in the reticulo-endothelial system, we have a mechanism which would become loaded with M-substance, and unless its production stopped almost entirely so that a prolonged period of detoxication occurred, the patient would remain ill.

Lovett Doust (23) has already shown that there is some peculiarity in the oxygen carrying capacity of the red cells in schizophrenia. While Boszormenyi-Nagy and Gerty (6) in Illinois have demonstrated that red cell metabolism is disturbed, he considers that something is wrong with the phosphorylating process.

It is also possible that the alveolar tissue of the lung, with its immense surface area covered with mucin and other potent substances, may in the presence of high concentrations of oxygen produce some unknown changes in some adrenalin derivatives. Whatever the explanation, there is clearly a rich field for research calling for immediate attention.

3. Toxic States and Schizophrenic-Like Conditions

Mr. Kovish's experience brings up the old question of the difference between "toxic" states and "true" schizophrenic like conditions. Where are we to pigeon-hole Mr. Kovish? In a sense this was a toxic state resulting from the regular admission of wine-coloured adrenalin. Yet in no sense does it resemble a toxic state of confusion. There was no confusion, no disorientation and no evidence whatever of clouding of consciousness, through the whole appalling five to six weeks. He said of this period, "Life did not have its full significance—I did not know whether I was awake or asleep." This is very close to Jung's (19) description of schizophrenia as a waking dream, or that excellent and evocative metaphor of Menninger's (25) "cold delirium". Attempts to differentiate between schizophrenic and toxic confusional conditions on an either/or basis suggest the possibility of a precision which for all its exact pretensions

is unsound and unscientific. What we are dealing with is a continuum with clouding and confusion at one end and disturbances of thinking and mood at the other, with changes in perception which can sometimes be common to both and may be absent from either. In such a continuum overlap is likely. Mr. Kovish's experience lies at the end without confusion and clouding, and so resembles that "cold delirium" or "waking dream" called schizophrenia. This simple notion, long ago noted by Louis Lewin (22) can save endless futile controversy as to whether some response is schizophrenic-like or not. The illness schizophrenia is in our view an end product of an interaction between a basic disturbance in brain function and the biopsychological endowment of the particular sufferer seen in a unique sociocultural setting. Such an illness could not be produced artificially for ethical reasons. Indeed as Johnson (18) has pointed out, to attempt to do so would be a form of criminal poisoning. Only in rare accidents of this sort can we expect to unravel some of the knotted strands.

Apart from such weighty matters, this story may help internists to pick up some similar cases and we trust that asthmatics will view their adrenalin with increased respect and caution.

So far those wishing to use the newer psychotomimetics have been prevented by the great difficulty in synthesizing them. Now, however, the sceptic or the enthusiast will not be incommoded by this and doubt can be speedily allayed. A bottle of 1:100 adrenalin solution, a few drops of hydrogen peroxide, a kitchen saucepan to speed up the change from adrenalin to adrenochrome, etc., a deVilbiss or other atomizer and a reasonable amount of caution is all that is necessary to prove or disprove our contention. We would emphasize that caution is essential, a proper recording apparatus desirable, that the effects may be subtle and prolonged and that if one's wife has a sick child, it is wise to have a colleague in to keep an eye on one. We hope in the near future to be publishing a method of preparing adrenochrome and adrenolutin which will make them more readily available than presently.

4. *The Significance of the Whole*

The involuntary prolonged experiment which Mr. Kovish made with some adrenalin catabolites and our own much less ambitious ones provide one with plenty to think about.

Changes in perception have been ascribed rather arbitrarily to prior changes in thought or mood. This seems to have sprung from Kraepelin's (20) dictum that perception was not disturbed in dementia praecox. True to his master, the great Wundt, Kraepelin made a distinction between perception and apperception. Wm. James (17) strongly disagreed with this separation which he held was misleading and artificial and his views seem to have only begun to percolate into psychiatry recently. So that psychiatrists have been comparatively unconcerned about disorders of perception in schizophrenia. Lately, refined testing techniques have shown perceptual anomalies in less acutely ill schizophrenic people but the most ill patients are still more or less inaccessible to the meticulous and lengthy testing necessary. The phenomenologist (37) and existentialist (9) psychiatrists in Europe and the widespread work with psychotomimetic agents of the last few years are now drawing more attention to perceptual disorders and the dismay and anxiety which they can easily provoke.

Mr. Kovish had unusual perceptions shortly after inhaling his wine-coloured adrenalin and later that evening they may have been the cause of his driving off the road. He continued to be plagued by them until some time after

he stopped all adrenalin. They were difficult to describe and he thinks it was the uncertainty which they engendered which disturbed him so much.

Language has been developed as a means of communicating socially recognized experience; experience which has no social recognition will not be represented by symbols allowing verbal communication, and in consequence it has no social reality. Language, as Whorf (39) has shown, restricts us to certain ways of describing our world which are usually effective in limiting what we perceive. This limitation is not however absolute, so that when we perceive something for which our language makes no provision we usually attempt to describe it in terms of something with which we and others are familiar. However, it may be that this does not communicate the experience because it is culturally unacceptable, so that the person whose perception differs from what is usual will, unless he is a deviant, become anxious and pre-occupied. Mr. Kovish could find no adequate way of rationalizing and so verbalizing these inexplicable happenings. The only explanation with which he was acquainted was a psychoanalytic one, so with some reservations he felt bound to consider it. A natural readiness to consider any explanation for inexplicable occurrences makes one sceptical of some of the formulations which psychoanalysts have elaborated to account for the genesis and symptomatology of psychotic illnesses. Have they forgotten the common human failing of demanding some explanations for weird happenings of any sort? Everyone likes to be able to communicate experience to others, and the schizophrenic person, aware that what he perceives no longer tallies exactly with language, is glad to use any means which will allow his experience to be clothed in the decent swathings of language.

The phenomenologist and existentialist schools of psychiatry under Binswanger's aegis are trying to bridge this gap by a deeper understanding of schizophrenic experience. Neither of us is particularly imaginative or sensitive, but a series of experiments with a variety of psychotomimetic agents during the last six years has made us less inclined to retreat into scepticism or to trot out some ready "explanation" when confronted with a story such as that of Mr. Kovish. We have learnt to listen respectfully.

Mr. Kovish did not begin to despair until at least a month had passed, in spite of disturbances in perception, morbid fears, sleep difficulties and low spirits. Weinberg (38), in a series of acute cases of schizophrenia, suggests that it takes from one to five months before the sick person comes to hospital. Presumably when onset is even more insidious, the patient may take much longer to come to hospital, and develop ways of rationalizing his experiences which make contact, treatment and re-ablement far more difficult. These rationalizations may also of course be extremely destructive to his relationships with other people and so further hamper his return to the community.

We have already noticed and made some suggestions regarding the lag between stopping all adrenalin and complete recovery. Mr. Kovish lays emphasis on having what he feels is an adequate explanation for his misfortune. He was incredulous "that this could happen to me—I'd always looked on those who gave way to nerves as being weaklings". After it had happened, even after he had made a complete recovery, as he was showing signs of doing, he would, he thinks, have always been wondering "whether this or that could bring it on again". Those who have themselves endured some of the bewilderment and uncertainty which an experience of this sort engenders will recognize what Mr. Kovish means. Luckily the passage of time dulls the vividness of it and self confidence increases. Yet how many recovered mentally ill people are

haunted by the ghosts of their illnesses, which constitute in themselves a major source of worry and insecurity.

Work with inhaled adrenolutin and other so far unspecified adrenalin derivatives has promise. The special value of the lung route, if we are correct, lies in its effect being much greater and more dramatic than the intravenous, but less likely to result in accumulation in the body, probably in the red blood cells from which it is slowly released over a period of time, producing insidious psychological changes, particularly when the subject experiences emotional disturbance. The lung route may be the nearest we can approach to the intraventricular instillation, and so offers the opportunity for a variety of experiments with psychotomimetics, both old and new. It may also provide clues as to how they work, some doubtless will show no more activity by lung than in other ways—some are much more powerful.

The adrenalin derivatives inhaled by H.O. did not give the familiar effects of adrenochrome or adrenolutin, neither did they resemble those of adrenalin itself. There was some similarity to his catatonic-like experience with *ololiuqui* (27) though the bowel discomfort and the extreme weakness was not present with the chief narcotic of the ancient Aztecs.

One could elaborate these speculations, but this is simply an account of the fortunate mishap which overtook Mr. Kovish and some experiments which it engendered. We hope that others will be encouraged to seek answers to some of the many questions which it raises. It may be possible to implant an indwelling catheter in the trachea of an experimental animal, with a covered diaphragm of the sort that Feldberg and Sherwood (10) have used in their cats, and so introduce to the lungs a variety of adrenalin derivatives in aerosol form and observe the behavioural changes. Toxic substances reaching the brain via the blood stream may produce a more exact model than those instilled into the ventricle. We may find that some fractions of adrenalin catabolites will produce very different changes from others. It would be unwise to forget that while our immediate attention has been on adrenalin, aberrant derivatives of nor-adrenalin and other precursors may be equally involved.

Finally Mr. Kovish reports the virtual disappearance of the asthma which afflicted him for so many years. A year after his ordeal he no longer takes any adrenalin and has no need of it. Psychosomatic problems of this sort are peripheral to our immediate interests so that we have not dealt with these aspects of the matter fully. One does not require much imagination to see some fascinating questions to ask. Has Mr. Kovish, inadvertently, achieved some permanent and beneficial change in his adrenalin metabolism? If this has happened (and we have only the merest hint of it), then perhaps the same principles could be used in a less drastic way to help other sufferers from asthma. We can only ask a question and hope that those who are especially concerned by these matters will be tweaked into trying to answer it.

These are a few prospects which lie ahead. They suggest that we now have tools allowing us to delve deeper into schizophrenia. As we do so we can hardly avoid a greater respect, understanding and sympathy for our patients as they struggle in loneliness against a world disintegrating around them.

SUMMARY

The authors describe the experiences of an asthmatic man who inadvertently inhaled at least 50 milligrams of "wine-coloured" adrenalin over a period of about four weeks. He had disturbances in perception, thinking and mood which convinced him that he was "going out of his mind". After he had stopped taking the adrenalin he felt better but not fully recovered. About six weeks after he had begun using this sample of adrenalin he chanced to read an article describing the effects of adrenochrome in a volunteer, which he considered very similar

to his own experience. This information coincided with a marked improvement in his condition.

After giving accounts of experimental admission of adrenolutin and mixed adrenalin metabolites by inhalation, the authors discuss the implications of these findings. They suggest that it may well be that the red cells act as a depot for some of these derivatives of adrenalin, and this might account for the different effect of the same substance when swallowed, given by vein, or inhaled. Commercial adrenalin for inhalation rarely goes pink, far more often it is brown or black. Asthmatics are well advised to beware of wine-coloured adrenalin.

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REFERENCES

1. ABOOD, L., Conference on Biochemistry and Mental Illness, University of British Columbia, 19-21 June, 1957.
2. ABRAMSON, H. A. (Ed.), *Neuropharmacology, Second Conference* 1955, 1956. New York: Josiah Macy, Jr., Foundation.
3. BAIN, W. A., GAUNT, W. E., and SUFFOLK, S. F., *J. Physiol.*, 1937, **91**, 233.
4. BEERS, C. A., *A Mind that Found Itself*, 1936. New York: Doubleday.
5. BOESEN, A., *The Exploration of the Inner World*, 1936. New York: Harpers.
6. BOSZORMENYI-NAGY, I., and GERTY, F. J., *Amer. J. Psychiatry*, 1955, **112**, 11.
7. CRONHEIM, G. E., Personal communications, 1956 and 1957.
8. DE JONG, H. H., *Experimental Catatonia*, 1945. Baltimore: The Williams and Wilkins Company.
9. ELLENBERGER, H. F., "Phenomenology and Existential Analysis". Lecture given 19 December, 1956 at the Institute Albert Prevost, Montreal. To be published in the *J. Can. Psychiatric Association*.
10. FELDBERG, W., and SHERWOOD, S. L., *J. Physiol.*, 1954, **123**, 148.
11. FITZHERBERT, J., *Brit. J. Med. Psychol.*, 1955, **28**, 191.
12. HENNEL, T., *The Witnesses*, 1938. London: Peter Davies.
13. HOFFER, A., *J. Clin. Exper. Psychopath.*, 1957, **18**, 27.
14. *Idem*, *Tranquilizing Drugs* (Ed., H. E. Himwich), 1957. Washington, D.C.: Amer. Assoc. Advancement of Science, Publ., 46.
15. *Idem*, *Hormones, Brain Function and Behavior* (Ed. H. Hoagland), 1957. New York: Academic Press.
16. *Idem*, OSMOND, H., and SMYTHIES, J., *J. Ment. Sci.*, 1954, **100**, 29.
17. JAMES, W., *Psychology (Briefer Course)*, 1948. Cleveland: World Publishing Company.
18. JOHNSON, D. MCL., *The Hallucinogenic Drugs*, 1953. London: Johnson.
19. JUNG, C. G., *Psychology of Dementia Praecox*, 1906. Zurich. Trans. Bull. A. Nervous and Mental Disease Monographs, 1936.
20. KRAEPELIN, E. (Barclay, R. M., Trans.), *Dementia Praecox*, 1919. Edinburgh: Livingstone.
21. LEIMDORFER, A., *J. Pharmacol.*, 1950, **98**, 62.
22. LEWIN, L., *Phantastica*, 1931. New York: Dutton.
23. LOVETT-DOUST, J. W., *J. Ment. Sci.*, 1952, **98**, 143.
24. MARRAZZI, A. S., *Ann. New York Acad. Med.*, 1957, **66**, 496.
25. MENNINGER, K. A., *The Human Mind*, 1949. New York: Knopf.
26. OGDON, J. H., *The Kingdom of the Lost*, 1947. London: The Bodley Head.
27. OSMOND, H., *J. Ment. Sci.*, 1955, **101**, 424.
28. *Idem*, *Neuropharmacology, Second Conference*, 1955, 1956. New York: Josiah Macy, Jr. Foundation.
29. PARFITT, D. N., *J. Ment. Sci.*, 1956, **102**, 429.
30. SCHENK, E., *The Book of Poisons*, 1956. London: Weidenfeld and Nicolson.
31. SCHREBER, D. P., *Memoirs of my Nervous Illness* (Translated, Macalpine, I., and Hunter, R. A.), 1955. London: Wm. Dawson.
32. SCHWARZ, B. E., WAKIM, K. G., BICKFORD, R. G., and LICHTENHELD, F. R., *Archives Neurology and Psychiatry*, 1956, **75**, 83.
33. SECHEHAYE, M., *Autobiography of a Schizophrenic Girl*, 1951. New York: Grune and Stratton.
34. SHERWOOD, W. K., Soc. Biol. Psychiatry Meeting, Chicago, Illinois, April, 1956.
35. STOLL, W. A., *Schweiz. Arch. f. Neurol. Psychiatry*, 1947, **60**, 279.
36. SZARA, S., *Experientia*, 1956, **12**, 441.
37. VAN DER BERG, J. H., *The Phenomenological Approach to Psychiatry*, 1955. Springfield: Thomas.
38. WEINBERG, S. K., *American Sociological Review*, 1950, **15**, 600.
39. WHORF, B. L., *Language, Thought and Reality*, 1956. New York: Wiley.

THE DIFFERENTIATION OF DELIRIUM TREMENS FROM IMPENDING HEPATIC COMA*

By

ESTHER A. DAVIDSON, M.B., M.R.C.P.E., D.P.M.

*Research Fellow in Psychiatry
Harvard Medical School
Research Associate in Psychiatry
Boston City Hospital*

and

PHILIP SOLOMON, M.D.

*Assistant Clinical Professor of Psychiatry
Harvard Medical School
Physician-in-Chief for Psychiatry
Boston City Hospital*

INTRODUCTION

THE problem of differentiating impending hepatic coma from delirium tremens is one which arises less commonly in Great Britain than in the United States. It is a problem which occurs, of course, chiefly in connection with alcoholic patients and while alcoholism has declined in Great Britain it is becoming an increasing medical and social problem in certain sections of the United States (1). Thus, at the Boston City Hospital alcoholism was a factor in over 45 per cent. of patients with cirrhosis of the liver at post-mortem (2). At the Hammersmith Hospital in London alcoholism was an aetiological factor in 5 per cent. of patients with the clinical diagnosis of cirrhosis (3). There is no doubt that the problem of differentiating impending hepatic coma from delirium tremens is a common and relatively easy one in Boston, whereas in London it is a rare and therefore more difficult one.

The importance of differentiating these two conditions lies in the field of therapy, for what is appropriate therapy for the one condition may be lethal for the other. Treatment by tranquilizers and sedatives may precipitate terminal hepatic coma in the patient in whom coma is only impending. Even small doses may be lethal. Hydration therapy required for delirium tremens may also be fatal to the patient with impending hepatic coma. A high protein diet for supposed delirium tremens may precipitate a patient with impending hepatic coma into terminal coma. On the other hand, purging and a protein-free diet, appropriate for impending hepatic coma, may be disastrous for the patient with delirium tremens. It is therefore vital to the patient's welfare that the physician be able to distinguish between delirium tremens and impending hepatic coma. It is also important to recognize that both conditions may exist simultaneously.

REVIEW OF THE LITERATURE

Delirium tremens: "Laerian who lay sick in the house of Demaenatus was seized with fever after drinking. Third day acute fever, trembling of the head, particularly of the lower lip; after a while convulsions, complete delirium, an

* From the Psychiatry Service, Boston City Hospital and the Department of Psychiatry, Harvard Medical School.

uncomfortable night." So wrote Hippocrates (4) of a patient who may have been the first recorded case of delirium tremens. In 1813 Thomas Sutton (5), an English practitioner, first distinguished delirium tremens from other types of "brain fever" and suggested its relationship to chronic alcoholism. He said it occurred only in individuals "who had been drinking beyond moderation or propriety". In 1822 Hayward (6) attributed the condition to venous congestion of the brain, and nine years later Wake (7) confirmed Hayward's findings and observed that the prognosis depended on the severity of associated injuries or disease processes. Kraepelin (8) gave the classic description of delirium tremens as follows: disorientation in the three fields, "collectedness" and ability to converse normally, good memory for past events, mingling of false and true perceptions, vivid hallucinations of sight, half-apprehensive, half-humorous mood, restlessness, tremors, and the odour of alcohol. He believed that this clinical picture was pathognomonic of delirium tremens.

Wortis (9), in reviewing the post-Kraepelinian literature, mentioned that at various times psychogenic, metabolic, and personality factors have been implicated in the aetiology of delirium tremens. In recent years the withdrawal theory for the production of delirium tremens has gained prominence (10), but remains to be proved.

Under the heading of "the tremulous-hallucinatory-epileptic-delirious states", Victor and Adams (11) recognized the following syndromes: (1) alcoholic tremulousness, which constituted four-fifths of the group; (2) alcoholic hallucinosis; (3) alcoholic epilepsy; (4) Wernicke's encephalopathy; (5) typical delirium tremens, which is the rarest of these syndromes.

The last is characterized by psychomotor, speech, and autonomic over-activity, disorientation, confusion, and disordered sense perception.

Wortis gave the incidence of delirium tremens as 39.8 per cent. of all psychotic admissions to the Boston Psychopathic Hospital in 1940. In 1951, at the Boston City Hospital, 11,987 patients were admitted to the medical services. Of these, 1,622 (14 per cent.) suffered from some form of acute alcoholism; 236 of these (2 per cent. of the total, 15 per cent. of the alcoholics) had delirium tremens on admission.

The biochemical or metabolic disturbances underlying delirium tremens are not known but Fink (12) has recently claimed that low serum magnesium levels are frequently found in this condition. He believes that the syndrome of muscle tremor, twitching, bizarre movements, convulsions, anxiety, agitation, sweating, fever, tachycardia and severe delirium occurs in various states as well as in delirium tremens as a result of magnesium depletion secondary to inadequate magnesium intake.

Impending hepatic coma: In ancient cultures the liver was treated with the respect due to the seat of the soul (13) and in later eras its secretions were considered important in the differentiation of personality types on a humoral basis (14). The ancients recognized disturbed mental states complicating acute jaundice but it was not until the 18th century that the protean manifestations of hepatic coma were described. Excitement, delirium and a tendency to maintain abnormal postures were noted by early observers (15, 16) as associated symptoms in acute fatal jaundice, while Bright, in 1836 (17) described the monotonous speech and flexion attitudes which preceded coma. Bright was the first to recognize that delirium and tremor might occur in patients with hepatic cirrhosis as did Frerichs (18), but the tendency was to regard these as incidental symptoms of this disease or to ascribe them to alcohol as did Copland (19) and Rolleston (20).

The neurological signs of increased muscle tone (21), reflex changes (20), and extensor plantar responses (22) and ankle clonus (23) were added to the clinical picture. Green (24) stressed the important differences between the coma of hepatitis and of cirrhosis; the former characterized by abrupt onset, excitement and delirium foreign to the latter. Choreiform movements were first mentioned by Stokes, Owen and Holmes (25).

Zillig (26) and Adams and Foley (27) gave a comprehensive account of these characteristic features of hepatic coma and the latter stressed the diagnostic importance of the "flapping" tremor and EEG changes. Sherlock *et al.* (28), showed that the syndrome could exist in chronic form and introduced the concept of portal-systemic encephalopathy, relating the neuro-psychiatric disorder to a combination of hepato-cellular disease of varied aetiology, shunting of portal blood to the systemic circulation and the presence of nitrogenous substances in the gut (Fig. 1). Sherlock and her colleagues (29) also

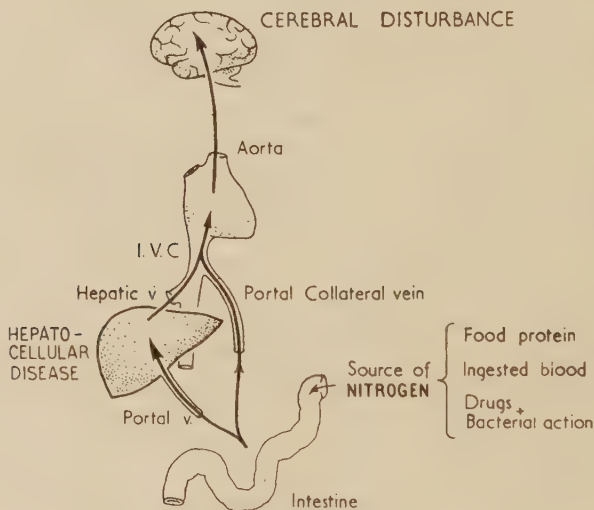


FIG. 1.—The mechanism of portal-systemic encephalopathy (Sherlock *et al.*, *Lancet*, ii, 453, 1954).

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showed that patients with the chronic syndrome may first be seen as diagnostic problems in mental hospitals, since mental disorder may dominate the clinical picture. Impending hepatic coma, no less in its acute than in its chronic form, can be regarded as the result of intestinal auto-intoxication by nitrogenous substances. It is indeed the toxic psychosis conceived of by Wagner-Jauregg (30) and elaborated on by Quastel (31).

MATERIALS AND METHODS

At the Hammersmith Hospital, London, 68 patients with hepatic coma or pre-coma, and one with hepatic coma and delirium tremens combined, were seen. More recently, at the Boston City Hospital, 15 patients with delirium tremens, 15 alcoholic patients with hepatic coma or impending hepatic coma, and 4 patients with both delirium tremens and impending hepatic coma were studied. Neuropsychiatric examination was carried out according to the schema

of Mayer-Gross and Guttmann (32). Case histories, data of physical status and results of laboratory tests were also acquired.

The neurological and psychiatric symptoms and signs were analysed in an attempt to delineate a characteristic picture for each syndrome. The details of this study and the actual statistical material have been omitted from this paper for the sake of brevity. It is planned that these will be published separately.

RESULTS

1. CLINICAL DIFFERENTIATION

(a) *Psychiatric Symptoms*

Delirium tremens and impending hepatic coma are both neuropsychiatric syndromes which may complicate chronic alcoholism. Each may follow a period of heavy alcoholic ingestion, but delirium tremens bears a more constant relationship to cessation of drinking, the symptoms occurring on the average three to four days after abrupt cessation. The alcoholic patient with impending hepatic coma may go on drinking even after he has become jaundiced and continue until he develops complete hepatic coma.

The patient with delirium tremens is fully conscious in that he responds to stimuli unless he has been sedated or tranquillized, in which case he may show impairment of consciousness similar to that which is an early manifestation of hepatic coma. Psychomotor activity is greatly increased in the patient with delirium tremens, whereas the patient with impending hepatic coma is usually akinetic with episodes of restlessness. Aggressive, destructive behaviour is much more common in the patient with delirium tremens. Hallucinations mainly of sight and hearing are invariable in delirium tremens but uncommon in impending hepatic coma, apart from simple visual hallucinations such as flashing lights and stars. When formed visual hallucinations occur in impending hepatic coma they usually take the form of simple wish-fulfilling fantasies such as seeing departed relatives. In delirium tremens the hallucinatory content is almost always of a terrifying nature and frequently involves unpleasant visions of animals. Anxiety and fear are prominent affects in the patient with delirium tremens but very uncommon in the patient with impending hepatic coma. The latter may show a euphoric or depressed mood, a genial euphoria being more common in the alcoholics with impending hepatic coma. At a later stage mental torpor and the desire not to be bothered distinguish the patient with pre-hepatic coma from the delirious alcoholic patient who is over-responsive to stimuli.

Occupational delirium as well as fragments of drinking behaviour such as ordering drinks, reaching for a glass, sipping movements are frequent components of delirium tremens. We have never observed such activities in impending hepatic coma.

(b) *Neurological Signs*

Tremor, not usually present at rest but present on intentional movement, characterizes both delirium tremens and impending hepatic coma. The tremor of delirium tremens is fine to coarse and rhythmic, while that of impending coma is typically irregular and "flapping". The "flap" may be absent in the alcoholic patient with impending hepatic coma. The speech in delirium tremens is rapid and elided. Sudden bursts of shouting in response to hallucinations interrupt conversation with the patient who is developing delirium tremens. The speech in impending hepatic coma is slow, slurred, and monotonous.

Perseveration of speech and aphasia, features of hepatic coma, have not been observed in delirium tremens.

Motor power is decreased in the patient with impending hepatic coma in association with pyramidal tract signs and possibly with a local disorder of muscle metabolism. Early symptoms of impending hepatic coma include complaints of weakness or easy fatiguability. At a later stage muscle weakness becomes intense and is associated with rigidity usually of a clasp-knife variety. Finally, flaccidity supervenes in terminal coma.

In delirium tremens the patient appears to be possessed of great muscular strength. Poised as he is for fight with or flight from the terrors that assail him, he is capable of muscular feats which at other times are beyond him. One patient with incipient delirium tremens was able to elude his real and imaginary pursuers in a dash through the streets of Boston, while another stated that he had broken the record for the mile run in his flight to the hospital. Exhaustion from over-activity is the greatest danger to life in delirium tremens.

Posture is unremarkable in delirium tremens except that the patient may not be able to maintain the standing position on account of tremor. Flexion attitudes are commonly seen in the patient with impending hepatic coma and a mask-like staring expression may be found in this condition. There is no obvious change in muscle tone in delirium tremens; in impending hepatic coma increased tone in the flexors is generally found. The tendon reflexes in the patient with delirium tremens usually show no change, though the ankle jerks may be absent if there is a peripheral neuropathy. On the other hand, the tendon reflexes are markedly increased in the patient with impending hepatic coma. Clonus develops irregularly and is associated with flexor plantar responses in the early stages, later with unilateral and then bilateral extensor plantar responses.

Sudden alcohol withdrawal following a period of heavy ingestion may produce symptoms of autonomic imbalance, premonitory to the development of alcoholic hallucinosis or delirium tremens. The earliest complaints are of restlessness, tension, and anxiety with chills and fever, and some degree of tremulousness, the "shakes". The face is flushed, there is tachycardia and sometimes auricular fibrillation. The appearance of cold sweats may coincide with relief from the anxiety and complete the withdrawal syndrome or the condition may progress. During the onset of delirium tremens the somatic accompaniments of fear may develop in association with hallucinatory episodes. One patient reported that he awoke to find his hair literally standing on end and immediately thereafter experienced frightening hallucinations. Such episodes may be combined with aggressive behaviour or with such precipitous flight from the terror that death may inadvertently occur.

The patient with impending hepatic coma slips into stupor usually without evidence of anxiety or fear. He is drowsy, though easily aroused, and appears preoccupied. His replies are short and polite and almost automatic. Low-grade fever may develop during episodes of coma and gross hyperthermia has been seen during terminal states. The patient with severe liver disease loses his ability to sweat. However, there is not the evidence of marked autonomic disturbance that is seen in the patient with delirium tremens.

Total insomnia is the rule in untreated delirium tremens with nocturnal exacerbation of symptoms, perhaps related to decreased sensory input (32). In impending hepatic coma hypersomnia gives place to reversal of sleep rhythm. Episodes of confusion with nocturnal wandering occur, but lack the affective

fire associated with the psychomotor discharges of the patient with delirium tremens.

Anorexia is a symptom of the alcoholic on spree as well as a major symptom of delirium tremens. The thirst for alcohol represents the only remnant of appetite. Fluids may be taken avidly if they are proffered as alcoholic beverages. In chronic impending hepatic coma in the non-alcoholic patient hyperphagia is common, sometimes as a perseveration of eating, as in the patient who devoured a complete round of sliced cake and rapidly became comatose, or as a vegetative symptom. Anorexia is found in the alcoholic patient with acute hepatic decompensation, and return of the appetite is always to be regarded as a good prognostic sign.

(c) General Medical Symptoms

The patient with delirium tremens is seldom visibly jaundiced although he may show slight elevation of the bilirubin level. The alcoholic patient with impending hepatic coma is usually deeply jaundiced as the result of acute hepatic decompensation following prolonged heavy drinking and poor diet. Fourteen of 15 patients with uncomplicated delirium tremens showed mild degrees of hepatomegaly in contrast to the hepatomegaly of the alcoholic in impending coma which was of gross degree and was usually associated with ascites and other evidences of acute hepatic decompensation. Splenomegaly is uncommon in impending hepatic coma of the alcoholic except in the more chronic types of hepatic coma where a portal-systemic shunt has developed, whereas it is common in the impending hepatic coma of the non-alcoholic.

The stigmata of liver disease; spiders, palmar erythema, loss of body hair, clubbing of the fingers, white nails, and multiple bruises, may be found in both delirium tremens and impending hepatic coma but such stigmata are likely to be more extensive in hepatic coma. In the alcoholic with liver disease, parotid enlargement, Dupuytren's contractures, pancreatitis, and macrocytic anaemia are frequently found and in the absence of a history, may distinguish the alcoholic from the non-alcoholic type of cirrhosis. Foeter hepaticus may be a feature of impending hepatic coma, but is never found in uncomplicated delirium tremens.

A condensation of the more useful distinctions in the clinical differentiation of delirium tremens from impending hepatic coma is presented in Table I.

TABLE I
Elements in the Clinical Differentiation of Delirium Tremens from Impending Hepatic Coma

							Delirium Tremens	Impending Hepatic Coma
Consciousness	..	..	..	..	..	..	+	—
Psychomotor activity	..	..	..	..	..	..	+	—
Anxiety	..	..	..	..	..	..	+	—
Tremor	..	..	..	..	..	..	Fine	Flap
Speech	..	..	..	..	..	..	Rapid	Slow
Hallucinations (formed)		..	..	..	..	..	+	—
Muscle strength	..	..	..	..	..	..	+	—
Autonomic imbalance	..	..	..	..	..	..	+	—
Jaundice	..	..	..	..	..	..	—	+
Sleep	..	..	..	..	..	..	—	+
Appetite	..	..	..	..	..	..	—	+
(+) Often present							(-) Rarely present	

2. LABORATORY DIFFERENTIATION

The laboratory can be of assistance in differentiating delirium tremens from alcoholic impending hepatic coma. Our data indicate that liver function tests show far more abnormality in alcoholic impending hepatic coma than in delirium tremens. The following tests were used: serum bilirubin, bromsulphthalein retention, prothrombin time, cephalin flocculation, and formol gel.

Additional laboratory findings were as follows: sodium, chloride, CO_2 combining power, and NPN were usually normal and potassium was usually low in both delirium tremens and in alcoholic impending hepatic coma. These findings, therefore, were not of help in the differentiation. Blood ammonia levels are usually elevated in alcoholic impending hepatic coma and normal in delirium tremens.

In the patients with both delirium tremens and alcoholic impending hepatic coma, liver function tests showed impairment similar to those patients with alcoholic impending hepatic coma alone.

Sensitivity to dietary protein is a diagnostic test for impending hepatic coma. If protein is ingested in gradually increasing amounts, there is no untoward effect in the patient with delirium tremens, but the patient with impending hepatic coma will become increasingly comatose as his protein tolerance is exceeded. A similar test may be done with ammonium chloride (29).

Recordings of electroencephalograms was not a routine procedure in the group of patients with delirium tremens. This procedure may be of value in differentiating delirium tremens from impending hepatic coma. Kennard *et al.* (33) have shown in delirium tremens low amplitude, increased amounts of fast activity, and low percentage of alpha rhythms. With symptomatic improvement the amount of fast activity decreased, slower alpha activity reappeared, amplitudes increased, and records became more regular. In impending hepatic coma in the Hammersmith series, slowing of the dominant rhythms from the alpha to the theta and delta ranges was associated with progression of coma. Although fast activity was seen it was by no means a prominent feature.

SUMMARY

The differentiation of delirium tremens from impending hepatic coma is important since appropriate therapy for the one condition may be lethal for the other. The literature concerning the two conditions is reviewed.

Our own material comprises, at the Hammersmith Hospital, London, 68 patients with hepatic coma or pre-coma and one with hepatic coma and delirium tremens combined, and, at the Boston City Hospital, 15 patients with delirium tremens, 15 alcoholic patients with hepatic coma or pre-coma, and 4 patients with both delirium tremens and impending hepatic coma. Clinical study of these 102 patients reveals data which is of value in the differential diagnosis.

The data from our patients is organized and presented in this paper under the following headings:

1. Clinical differentiation: (a) Psychiatric symptoms; (b) Neurological signs; (c) General medical symptoms and signs.

2. Laboratory differentiation.

The more useful clinical distinctions are condensed and shown in the form of a Table.

ACKNOWLEDGMENTS

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REFERENCES

1. KELLER, M., and EFRON, V., *Quart. J. Stud. Alc.*, 1955, **16**, 4, 619.
2. MACDONALD, R. A., *New Eng. J. Med.*, 1956, **255**, 1179.
3. Personal observation.
4. HIPPOCRATES (460-370 B.C.): Quoted by Wortis, H., (9) below.

5. SUTTON, T., *Tracts on Delirium Tremens, on Peritonitis and Some Other Internal Inflammations, Infections, and on the Gout*, 1813. London: Moyes.
6. HAYWARD, G., *New Eng. J. Med. Surg.*, 1822, **11**, 235.
7. WAKE, J., *Med. Commun. Mass. Med. Soc.*, 1831, **5**, 136.
8. KRAEPELIN, E., *Lectures on Clinical Psychiatry*, 1904. New York: Wood & Co., p. 120.
9. WORTIS, H., *Quart. J. Stud. Alc.*, 1940, **1**, 251.
10. *Med. Res. Mid-century Survey*, 1956, **2**, 537. Boston: Little, Brown & Co.
- WIKLER, A., PESCOR, F. T., FRASER, H. F., and ISBELL, H., *Amer. J. Psychiat.*, 1956, **113**, 106.
11. VICTOR, M., and ADAMS, R. D., "Metabolic and Toxic Disorders of the Nervous System", *Res. Publ. Ass. Res. Nerv. Ment. Dis.*, 1953, **32**, 526.
12. FLINK, E. B., STUTZMAN, F. L., ANDERSON, A. R., KONIG, T., and FRASER, R., *J. Lab. and Clin. Med.*, 1954, **43**, 169.
- SUTER, C., and KLINGMAN, W. O., *Neurol.*, 1955, **5**, 691.
13. BAUMANN, E. D., *Janus*, 1931, **35**, 153 and 185.
14. GALEN (131-200 A.D.): Quoted by Baumann, E. D.
15. CHEYNE, J., *Dublin Hosp. Rep.*, 1817, **1**, 273.
16. GRIFFIN, W., *London Medical Gazette*, 1834, **13**, 801.
17. BRIGHT, R., *Guy's Hospital Rep.*, 1836, **1**, 604.
18. FRERICH, R. T., "A Clinical Treatise on Diseases of the Liver", 1860. London: New Sydenham Society.
19. COPLAND, J., *A Dictionary of Practical Medicine*, 1858. London.
20. ROLLESTON, H., *Diseases of the Liver, Gall-bladder and Bile Ducts*, 1912. 2nd edition. Philadelphia.
21. JENKINS, E. M., *Brit. Med. J.*, 1884, *i*, 357.
22. ELLIOT, T. R., and WALSHE, F. M. R., *Lancet*, 1925, *i*, 65.
23. WILLCOX, W. H., *Trans. Med. Soc. Lond.*, 1919, **42**, 147.
24. GREENE, G. H., *Arch. Intern. Med.*, 1941, **67**, 867.
25. STOKES, J. F., OWEN, J. R., and HOLMES, E. G., *Brit. Med. J.*, 1945, *ii*, 642.
26. ZILLIG, G., *Arch. Psychiat. Nervenkr.*, 1948, **181**, 21.
27. ADAMS, R. D., and FOLEY, J. M., *Res. Pub. Ass. Res. Nerv. Ment. Dis.*, 1953, **32**, 198.
28. SHERLOCK, S., SUMMERSKILL, W. H. J., WHITE, L. P., and PHEAR, E. A., *Lancet*, 1954, *ii*, 453.
29. SUMMERSKILL, W. H. J., DAVIDSON, E. A., SHERLOCK, S., and STEINER, R. E., *Quart. J. Med.*, N.S., 1956, **98**, 245.
30. VON JAUREGG, W., *Wien. Klin. Wochenschr.*, 1896, **9**, 165.
31. QUASTEL, J. H., *Lancet*, 1932, *ii*, 1417.
32. MAYER-GROSS, W., and GUTTMANN, E., *J. Ment. Sci.*, 1937, **83**, 440.
33. LEIDERMAN, P. H., MENDELSON, J., WEXLER, D., and SOLOMON, P., *Clinical Aspects of Sensory Deprivation*. (Presented at Annual Meeting Amer. Med. Assoc., New York, June, 1957.)
34. KENNARD, M. A., BUEDING, E., and WORTIS, S. B., *Quart. J. Stud. Alc.*, 1945, **6**, 4.

A DOUBLE-BLIND CONTROLLED COMPARISON OF THE EFFECTS OF CHLORPROMAZINE, BARBITURATE AND A PLACEBO IN 142 CHRONIC PSYCHOTIC IN-PATIENTS

By

J. CRAWFORD LITTLE, M.B., Ch.B., M.R.C.P.E., D.P.M.(Dunelm)

Senior Registrar

Joint Department of Psychological Medicine

Royal Victoria Infirmary and University of Durham, Newcastle upon Tyne

INTRODUCTION

SINCE the original article by Elkes and Elkes (1954) numerous reports have been published in the world literature on the use of chlorpromazine in chronic psychotic in-patients. The majority of these studies indicate that the drug is indeed effective in improving the behaviour of these patients; a small minority of authors is more sceptical including Mitchell (1956) using doses up to 300 mg. daily, Sarwer-Foner and Ogle (1956) with doses of 150–400 mg. daily, and Trelles and Saavedra (1954) using sleep treatment. The literature contains numerous reports of controlled and uncontrolled studies on chlorpromazine and comparisons of its effects with those of a placebo, Reserpine, Azacyclonal, etc.; attention is now turning to the effects of such drugs in various combinations.

However, for many years barbiturates had been the main sedatives used in attempting to modify the behaviour and tension of chronic psychotic patients and it was felt it would be of value to compare the results of chlorpromazine therapy with those obtained by barbiturates, the traditional standby. This point was raised by Tewfik (1955) who wrote "chlorpromazine . . . must also be controlled against drugs producing equivalent sedative effects before the word 'special' as a description of its effect can be justified. Until this has been proved chlorpromazine should not be allowed to supplant the much less toxic cheaper sedatives in common use."

Although a review of the literature shows that the great majority of workers find chlorpromazine effective for chronic psychotic patients, nevertheless there is room for criticism of the methodology of many of the studies, a large number of which are not controlled, while in others the samples are too small to draw definite conclusions. In view of the increasing popularity of this expensive and potentially lethal preparation it was felt that a well-controlled study using an adequate number of patients was desirable, firstly to establish its effectiveness as compared with an inert placebo, and secondly to determine whether it has in fact a superior action to the well tried, less toxic and cheaper barbiturates. No study of this basic problem has so far been reported in the literature.

MATERIAL AND METHOD

Material

The project was carried out at St. Luke's Hospital, Middlesbrough.

It was decided to utilize as many chronic patients as possible in the hospital, but certain groups had to be eliminated—e.g. the infirmary wards, chronic

working patients not under observation and certain wards where other research was in progress. In all 5 wards were chosen, 3 male and 2 female, with a total of 142 patients, of whom there were 85 males and 67 females. One hundred and twenty-seven patients were certified and 15 under voluntary status.

TABLE I
Diagnostic Groupings

Reaction Type	Sub Group						Number of Patients
Schizophrenic	"Schizophrenia"	..	..	..	..	..	80
	Schizophrenia with mental deficiency	..	..	..	..	..	10
	Paranoid schizophrenia	..	..	..	..	..	12
	Catatonic schizophrenia	..	..	..	..	..	7
							109
	Paraphrenia	..	..	..	..	..	12
Affective	Chronic depressive	..	..	..	..	..	4
	Manic depressive psychosis	..	..	..	..	..	2
							6
Organic	Disturbed mental defective	..	..	..	..	..	2
	G.P.I.	..	..	..	..	..	7
	Senile and pre-senile dementia	..	..	..	..	..	3
	Epileptic psychosis with dementia (no recent fits)	..	..	..	..	..	2
							14
Neurotic	Anxiety state	..	..	..	..	..	1
	Total	..	..	..	..	..	142

TABLE II
Age Distribution

Extreme range 21-75 years:

Age groups (in years)	20-29	30-39	40-49	50-59	60-69	70-79
Number of patients	9	31	39	24	33	6

Total: 142 patients

Duration of In-patient Stay

Extreme range 6 months to 51 years:

Duration in years since first admission	Under 1 year	2-3 years	4-10 years	11-35 years	Over 35 years
Number of patients	3	13	38	83	5

Total: 142 patients

Of the 142 patients 74 had received E.C.T. with a recorded result and it had proved effective in some degree in 37 (50 per cent.). Of the 19 patients on maintenance E.C.T. this treatment was stopped in nine cases during the trial as the

nursing staff did not feel it was required. This was irrespective of the drug being used, and may be attributed to the beneficial psychological effect of the trial on patients and staff. Twenty-one patients had received insulin coma therapy with some improvement in 10 (in 3 this was temporary only), and leucotomy or topectomy had been carried out in 27 patients with some improvement in 13 (in 4 of these the effect was temporary only). All the cases of G.P.I. had received malaria, arsenic or penicillin in various combinations. These figures only refer to the results of previous treatment in patients who have not left hospital, and in no way reflect their general efficacy in psychiatric practice. Prior to the trial 55 patients, mostly on the refractory wards, were regularly receiving some sedative drug. It was not possible to withdraw these drugs entirely, but they were reduced to some degree a month before the trial started: 40 patients were receiving sodium amytal or nembutal in doses of 6-12 grs. daily; this was reduced to one half or one-third. The remaining 15 patients on small doses of 3 grs. per day of one or other of these drugs, or similar amounts of other sedatives, were allowed to continue as before.

A month before the start of the trial much sedation was gradually withdrawn. When this proposal was first suggested the nursing staff, particularly on the refractory wards, expressed considerable anxiety as to possible violence, thus most of the disturbed patients were only reduced to a smaller dose of sedative. After the first week, during which there was some excitement, both patients and staff settled down and there appeared to be very little difference in the general behaviour, which was assessed as a base line for comparative purposes during the trial.

Method

A double-blind controlled experimental situation was set up in the following manner: a survey was made of all patients in each ward and the infirm and epileptic were eliminated from the trial, several reports having suggested that chlorpromazine may aggravate a dysrhythmic tendency. Also excluded were three patients who refused to co-operate.

The remaining 142 patients were divided at random into 3 groups (I, II, III). As chronic patients were chosen for the study, their mental state being more or less stationary, it was feasible to use each patient as his own control. Messrs. May and Baker kindly prepared the following tablets in identical form: amylobarbitone gr. 1, chlorpromazine 25 mg. and an inert preparation for use as a control. It was decided to keep the duration of the trial as short as possible in order to minimize such variables as staff holidays and changes. To this end the standard duration for giving each drug was three weeks. It is known that barbiturates act fairly rapidly but it has been claimed that chlorpromazine does not exert its full action until it has been exhibited for a period of six weeks or so. In order to test this hypothesis, chlorpromazine was given for two consecutive periods (C1, C2). The drugs were given to the groups in the following order:

TABLE III

Group								Consecutive 3 Week Periods			
I	..	..	..	..	..	..	..	C1	C2	I	B
II	..	..	..	..	..	..	..	B	C1	C2	I
III	..	..	..	..	..	..	..	I	B	C1	C2

Where C1 = Initial 3 weeks on chlorpromazine
 C2 = Second 3 weeks on chlorpromazine
 I = 3 weeks on inert (placebo) tablets
 B = 3 weeks on barbiturate (amylobarbitone)

In conference with the charge nurse or sister of the ward, the mental state over the past three months was outlined and the significant clinical features noted. *An initial tension rating* was allotted on a 4-point scale:

- H — persistently high tension.
- HS — tension usually normal or low but high sporadically.
- N — normal tension.
- F — flattened or below normal tension.

In order to test the validity of these tension ratings as carried out by the senior nurse on each ward, the deputy matron and deputy head male nurse were independently invited to indicate their assessments using the same rating scale and a high degree of correspondence was revealed.

It was decided to use a dosage scale of two tablets of each preparation three times a day, i.e. amylobarbitone 6 grs., chlorpromazine 150 mg., or 6 inert tablets per day, working up to this maximum over four days in all cases. Each patient was allotted a pill box with his name on it and the investigator (J.C.L.) dispensed the drugs into these boxes according to the patient's treatment group (I, II, III). The boxes were then sent to the ward and the tablets given under the direct supervision of the charge nurse or sister at the standard rate of two tablets t.d.s. As chlorpromazine is known to cause immediate hypotensive effects in some patients, it was arranged that all patients should sit down for half an hour after taking the tablets. This also had the effect of concealing from the nursing staff the fact that some patients responded in this way and avoided the danger, from the experimental point of view, of one group being differentiated in a supposedly blind procedure. The only information available to the charge nurse or sister was the name on the box and the standard dosage schedule. It had been explained that various different tablets were being used but no information was given as to their nature.

The senior ward nursing personnel acted as assessors of the effects of treatment and kept detailed notes of any change in the mental state and of manifestations such as flushing, unsteadiness and drowsiness, and were warned to check for any signs of jaundice or infection, especially sore throat.

Initially, and at the end of each three-week period, the white blood count was estimated and pulse and temperature recording were carried out twice daily. Also at the end of each three-week period the investigator received a report from and questioned the appropriate assessor for: (a) drowsiness, (b) any other side effects, (c) changes in tension, (d) changes in any of the clinical features already listed, and (e) the general change in the patient from a nursing point of view. This was assessed on a 7-point scale: Very much worse ($\equiv$), Definitely worse ($=$), Slightly worse ($-$), No change (0), Slightly improved ($+$), Definitely improved ($++$), and Very much improved ($+++$). Any attempt at suggestion of expected effect on the part of the investigator was carefully held in check. This, however, was not considered likely to be a serious factor, since there were one hundred and forty-two patients in the trial and it was obviously impossible to remember which preparation a given patient was receiving at any given time. No definite criteria were laid down for the assessor's judgments, but the main points under consideration were changes towards normality—lessened aggression and more co-operation and sociability in the refractory patients, and more drive and spontaneity in the apathetic. The assessors tended to score adversely any excessive or prolonged drowsiness, their desire being to have their patients co-operative and active.

At the end of each three weeks trial period the tablets were changed by the

investigator and the next trial period commenced. Thus at no time was the person assessing the effects aware of the drug being used.

RESULTS

Of the 142 patients who started 137 completed the trial (1 elderly female patient died of perforated carcinoma of rectum; 2 patients were subjected to leucotomy towards the end of the trial; 1 refused to continue after 6 weeks; and 1 overstayed her leave).

The particular sequence in which a drug was given in the three groups is shown by the χ^2 test not to cause a significant difference at the 5 per cent. level in its overall effect. Further, a comparison of the effects of chlorpromazine after the initial 3 weeks (C1) with its effect after a total of six weeks (C2) shows that there is statistically no significant difference in the general effect at the 5 per cent. level. Thus in the following tables the results after 3 weeks treatment (C1) have been used for the comparison with the effects of the placebo and barbiturate. The comparison of the general effects of the three preparations is shown in Table IV and illustrated in histogram form in Figure 1.

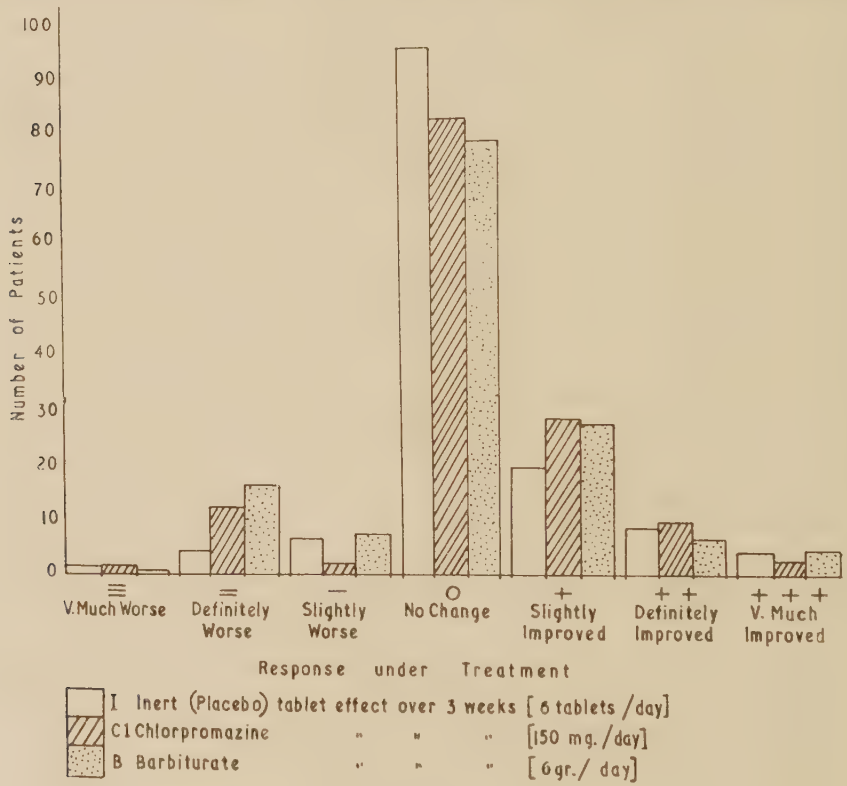


FIG. 1.

TABLE IV

General Response to Treatment in Total Group									
Preparation			Response to treatment						Total
		≡	=	—	0	+	++	+++	
I (inert tablet)	..	1	4	6	95	19	8	4	137
C1 (chlorpromazine)	..	1	12	2	82	28	9	2	136
B (barbiturate)	..	Nil	16	7	78	27	6	4	138

Figures refer to numbers of patients.

TABLE V
Analysis of Effect of the 3 Preparations by Initial Tension Ratings (H, HS, H + HS Combined, N.F.)

Tension Rating	α or β Grouping	On Inert Placebo Tablets Treatment Response										On Chlorpromazine (Cl) Treatment Response										On Barbiturate Treatment Response															
		Nil	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35
H ..	..	Nil	2	1	23	6	2	1																													
	α	3	3	23	9																																
HS	..	2	2	30	3																																
	β	1	Nil	4	24	7	Nil	Nil																													
H plus HS	..	5	5	24	7																																
	β	1	1	35	7																																
N ..	..	1	2	5	47	13	2	1																													
	α	8	8	47	16																																
F ..	..	3	3	65	3																																
	β	Nil	2	Nil	19	4	6	3																													
F ..	..	2	2	19	13																																
	α	2	2	23	9																																
F ..	..	Nil	Nil	1	29	2	Nil	Nil																													
	β	1	1	29	2																																
		Total 137										Total 136										Total 138															

Figures refer to number of patients.

In order to keep the figures large enough for statistical purposes, the responses under treatment can be grouped together in various ways; two alternatives have been chosen here: (1) α Grouping ($\equiv, =, -$) (0) (+, ++, +++), which compares overall worsening and improving effects with the "no change" response; (2) β Grouping ($\equiv, =$) ($-$, 0, +) (+, ++, +++), which groups minor degrees of change with "no change", and highlights the more marked degrees of altered behaviour. From the clinical point of view the latter is perhaps the more desirable, as we wish to induce quite definite changes in our patients rather than subtle and slight alterations which make little difference to the overall atmosphere of the ward. The χ^2 test using both alpha and beta groupings shows that in a comparison of the total results there is no significant difference between the general effect obtained with chlorpromazine, barbiturate and the placebo tablet. The total number improving with the chlorpromazine and barbiturate is almost identical, and there is no trend towards an advantage over the placebo using either the alpha or beta groupings.

The comparison of the general effect with each of the three preparations was broken down and considered separately for each of the four "Initial tension ratings" (H, HS, N, F). These results are shown in Table V using the α and β groupings of response to treatment, and are demonstrated in the form of a histogram in Figure 2.

It is readily seen that a more favourable response is occurring in the H and HS groups, and as the results in all groups show no difference between the three preparations, the chance of obtaining any significant effect must lie here. By eliminating from consideration the patients whose initial tension state was normal or flattened, and abstracting the figures for the 70 odd patients who show persistently or sporadically raised tension (H, HS) the difference between the drug responses is still not significant at the 5 per cent. level, but a trend in favour of the drugs as against the inert preparation is discernible.

Leaving out of consideration the worsening effects and concentrating entirely on the improving effects, the following results are obtained (see Table VI).

TABLE VI
Table of Comparison by Improving Effects Only

Preparation		Treatment Response Groups			
		(+++, +++)		(+, ++, +++)	
			Per cent.		Per cent.
(1) All Tension Groups	Placebo ..	12 out of 137	(9.0)	31 out of 137	(22.5)
	Chlorpromazine	11 out of 136	(8.0)	39 out of 136	(28.5)
	Barbiturate ..	10 out of 138	(7.5)	37 out of 138	(27.0)
(2) Groups H. and H.S. only	Placebo ..	3 out of 71	(4.25)	16 out of 71	(22.5)
	Chlorpromazine	7 out of 70	(10)	26 out of 70	(37)
	Barbiturate ..	6 out of 71	(8.5)	24 out of 71	(33.5)

None of these differences is significant at the 5 per cent. level.

Once again a moderate suggestion of improvement emerges in the groups with heightened psychological tension; this is, however, much less apparent when only considering the "marked improvement" groups (+++, ++++).

Although numerous "tranquillizing" drugs are now in general use, there are few indications as to which is likely to be most beneficial in a particular

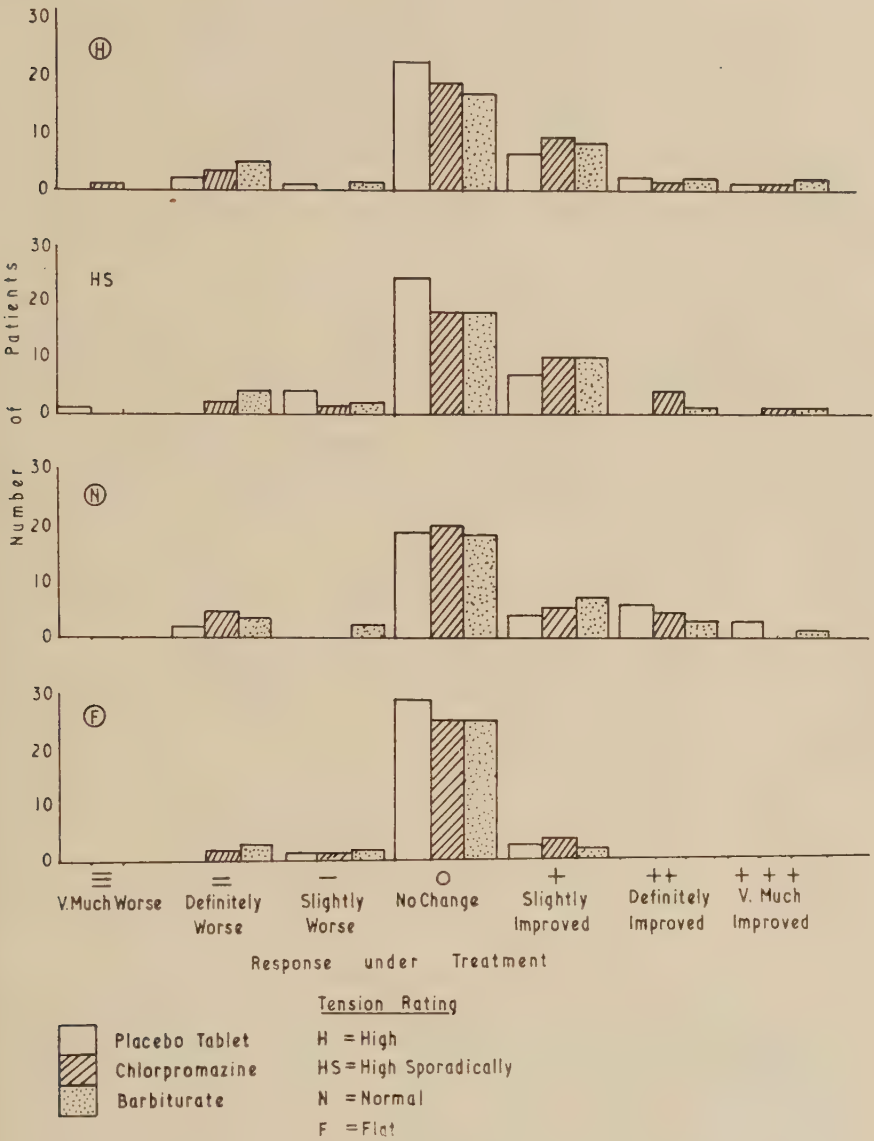


FIG. 2.

case. Thus a fairly detailed account is given of some findings in this trial related to this topic. Once it has been shown that equal *numbers* of patients improve with either of two treatments, there is still a possibility that the two groups are not identical and that some patients respond to one drug and not the other. In order to investigate this point a comparison of the two drugs (B and C1) was made for each patient in order to determine whether a preference was shown for one drug or the other. For the purposes of this assessment (—, 0, +) were regarded as being equivalent—this has been done in order to eliminate trifling differences in drug response. The results are shown in Table VII.

TABLE VII
Illustrating Drug Preference Between Barbiturate and Chlorpromazine

Tension Groups			Same Effect With Either Drug	Chlorpromazine More Effective Than Barbiturate (C1 > B)	Barbiturate More Effective Than Chlorpromazine (B > C1)	Total
N plus F	..	..	55	6	6	67
H plus HS	..	..	49	10	10	69
All cases	..	..	104	16	16	136

Thus: Over *all* cases the proportions showing a preference for B over C1 are not significantly different from the proportions showing a preference for C1 over B.

This also applies for tension groups H and HS combined and for groups N and F combined, but the proportion of cases showing a preference is higher in H and HS than in N and F (20 cases out of 69: 12 cases out of 67).

A search was made for any factors which might serve to differentiate the 16 cases showing a preference for C1 from the 16 preferring B. There was no striking difference in diagnostic groupings, age, and duration since first admission. One possible clue is in the response to previous E.C.T.: of the B preference group 14 patients had had E.C.T. with 10 responding favourably. In the C1 preference group 12 had received E.C.T. and 5 had responded favourably (this difference however, is not significant at the 5 per cent. level). The incidence of flushing and pallor were not significantly different in the two groups. By far the most striking result of this analysis of preference groups occurred among the female patients:

On one ward (ward 1), of 9 patients showing a preference 8 preferred B > C. Tension Ratings HS nil; H 4; N 3; F 1. On the other ward (ward 2), of 9 patients showing a preference 8 preferred C > B. Tension Ratings HS 6; H 2; N nil; F nil. All these 18 patients were schizophrenics.

These differences are highly significant and on attempting to explain them certain dissimilarities between these two female groups are apparent:

Ward 1 is a ward for young schizophrenics mainly. The average age of these 9 cases was 34½ years, and the average duration since first admission was 7 years, thus the average age at first admission was 27½ years. Much active treatment had already been carried out on this ward, 2 of these 9 patients having been leucotomized, 7 having received E.C.T. in the past (5 responding favourably in some degree) and 4 had had insulin therapy. Sedation was hardly used at all. The one patient from this ward who showed a preference for C > B was an older patient aged 41 with 9 years duration of stay, who had worsened on E.C.T. and required heavy sedation.

Ward 2 is a large refractory ward with rather older patients: the average age of these 9 patients was 45 years, and the average duration since first admission was 13½ years, thus the average age at first admission was 31½ years. None of these patients had been leucotomized or had received insulin therapy, but 6 had had E.C.T. previously—in only one was there a favourable response. Fairly heavy sedation was regularly taken by 6 of them. The one patient in this ward who preferred B > C differed only in that she had not been sedated at all previously.

The differences in favourable response to E.C.T. in the two groups (ward 1: 5 out of 7; ward 2: 1 out of 6) show a trend in favour of ward 1 which does not reach significant proportions however.

Caution is needed in interpreting these results. The figures are small and several of the factors noted have been based on subjective judgments on the ward sister's part, e.g. initial tension ratings and, to some extent, the effect of previous E.C.T. where it was not noted was based on the sisters' recollections.

Hence, the results would appear to suggest that among females at least, younger schizophrenic patients with various tension ratings but not given to violent outbursts, and who have responded well to E.C.T. in the past, show some preference for barbiturate sedation, whereas rather older schizophrenics given to violent outbursts and who have not improved with E.C.T. fare better on chlorpromazine. It may well be that the group on ward 2 are in fact catatonic schizophrenics—it should be noted that the average age on first admission in this ward was $31\frac{1}{2}$ years, as against $27\frac{1}{2}$ years in the other ward. Although aggressive outbursts were common with this group, no stuporose episodes were reported and statuesque posturing and *flexibilitas cerea* were not seen. This, however, may not be very surprising as many catatonic patients lose these significant features over the years. This question of drug preference and its relations to diagnosis, behaviour and E.C.T. response requires further study. But for the most part patients who respond in one way to one of the drugs under trial will respond the same way, and equally, with the other.

An assessment of the *overall psychological effect* of the trial in this whole group of chronic in-patients may be made by considering the total effect of the inert tablets, as compared with the previous (pre-treatment) state. See Table VIII.

TABLE VIII

The Effect of the Inert Tablet (Placebo) Analysed According to Initial Tension Rating

		Treatment Response			Total
Initial Tension Rating		(Ξ , $=$, $\neg$)	(O)	(+, ++, +++)	
α Grouping	H plus HS	8	47	16	71
	N	2	19	13	34
	F	1	29	2	32
	All tension groups	11	95	31	137
Initial Tension Rating		(Ξ , =)	($\neg$, O, +)	(++, +++)	Total
β Grouping	H plus HS	3	65	3	71
	N	2	23	9	34
	F	0	32	0	32
	All tension groups	5	120	12	137

Thus using the *alpha* grouping a definite favourable effect on behaviour is seen in the group as a whole, most marked in the normotensive patients and absent altogether in the apathetic. With the *beta* grouping which is designed to reveal marked changes only, the favourable trend is only revealed in the normotensive group. Thus the psychological effect of the inert tablet only causes an overall degree of slight improvement, most marked in the group of patients whose tension is not markedly increased or decreased.

Useful results have been claimed for chlorpromazine in the treatment of various *paranoid psychoses*. The analysis of results in the group of twelve elderly certified paraphrenics is shown in Table IX and reveals that chlorpromazine and barbiturate in the stated dosage would seem to be singularly ineffective in this group.

TOXIC AND SIDE EFFECTS

These aspects have been commented on in numerous papers and are now well known, thus reference will only be made to a few special points.

Dry mouth is a rare complaint in chronic schizophrenics.

TABLE IX
Results in 12 Chronic Paraphrenics

Case Numbers	Sex	Age	Duration of In-Patient Stay in Years	Initial Tension Rating	Treatment Response with the Different Preparations			
					C1	C2	I	B
130	M	74	30	F	0	0	0	0
7	M	56	2	N	0	0	0	0
22	F	63	14	N	0	0	0	0
37	F	65	24	N	0	0	0	0
85	M	49	8	N	0	+	++	++
112	M	61	9	N	0	+	+	0
117	M	56	11	N	+	+	+	+
113	M	69	21	HS	+	+	+	+
118	M	57	11	HS	+	+	+	+
122	M	52	4	HS	+	—	—	0
51	F	69	30	H	+	+	+	+
99	M	53	21	H	+	++	++	0

Jaundice and Agranulocytosis. No patient in the trial had to stop treatment because of toxic effects. Two developed transient jaundice but were able to continue. None developed agranulocytosis, but after the trial was completed a 52 year old female in an admission ward suffering from acute hypomania, from which she was recovering well, developed cellulitis of the face although the fauces were quite healthy. She had been receiving chlorpromazine 150 mg. daily for seven weeks (total 7.5 grams). The white blood count on that day was 2,000 per cu.mm.; polymorphonuclear neutrophils 4 per cent. (actual number 80 per cu.mm.). Penicillin was prescribed and next day the W.B.C. was 1,200 cu.mm.; polymorphs 1 per cent. (actual number 12 per cu.mm.). A sternal marrow biopsy revealed the myelocyte representation to be within the normal range but there was a complete absence of mature granulocytes. Eight days later the W.B.C. was back to 6,000 per cu.mm. The patient complained of no pain and felt perfectly well throughout. This sudden emergency should serve as a warning to those of us who make a custom of telling our out-patients (especially those of poorer intelligence) to report if they develop a sore throat. Had this particular woman, well on the way to recovery, been an out-patient she may well have died.

Drowsiness. It has been claimed as one of the virtues of chlorpromazine that it causes less drowsiness than barbiturates. In the present trial drowsiness or its absence was specially noted during each treatment period (see Table X).

TABLE X
Presence of Significant Drowsiness

Preparation	Number of Patients
	Per cent.
Placebo (I)	15 (11)
Barbiturate (B)	28 (20)
Chlorpromazine (C1)	39 (28.5)
Chlorpromazine (C2)	24 (17.5)

Tests of significance on these results show: the difference in the numbers drowsy on I and B is significant. The difference between the numbers drowsy on I and C1 is highly significant, and the figure for C1 is also significantly

greater than that for B (using McNemar's statistical method (McNemar, 1947)). The difference between the numbers drowsy on C1 and C2 is also significant but the difference between I and C2 is not significant using McNemar's method. The difference between the numbers drowsy on B and C2 is not significant.

In other words chlorpromazine is associated with a high incidence of drowsiness (38.5 per cent.) during the first three weeks and this falls to an incidence subsequently ($17\frac{1}{2}$ per cent.) which is not significantly greater than that occurring with the inert tablet (11 per cent.). Barbiturate has been proved in this sample to be associated with an incidence of drowsiness (20 per cent.) significantly greater than that seen with the inert tablet. There is, however, no significant difference between the numbers of patients drowsy on barbiturate and chlorpromazine. These findings, although consistent with, do not support the general view that one of the virtues of chlorpromazine is a lesser incidence of drowsiness than with barbiturate.

Autonomic response. The only autonomic responses considered here are flushing and pallor which have both been reported as side effects of chlorpromazine therapy. Such responses occurred in 48 patients (34.5 per cent.) (see Table XI).

TABLE XI
Incidence of Autonomic Responses (Flushing and Pallor)

Preparation	Number of Patients		
	Flushed	Pale	Total
I (inert tablet)	16	1	17
C1 (Chlorpromazine)	18	7	25
C2 (Chlorpromazine)	16	6	23
B (Barbiturate)	12	1	13

Comparing the responses under this heading observed while on the drugs as compared with those observed while on the inert tablets, the only significant difference is between I and C1 with regard to pallor only. (Significant at the 5 per cent. level using Edward's modification of McNemar's method (Edwards, 1948).) Thus there is no evidence from this trial to support the view that flushing is a side effect of chlorpromazine therapy, but there is evidence that pallor is present to a significant degree during the initial three weeks of chlorpromazine therapy only, as compared with the period on inert tablets. No relationship was discernible between treatment response and these forms of autonomic reaction (flushing and pallor).

COMMENT

The principal conclusion derived from this trial, carried out on a reasonably large sample population of chronic psychotic in-patients, is that neither chlorpromazine nor barbiturate is shown to exert any effect significantly superior, from the nursing point of view, than can be obtained with an inert placebo tablet.

Other workers have used different criteria of the effect of drugs such as scoring methods based on the response under treatment of various symptoms. It is maintained here, however, that an assessment from the nursing standpoint is a valid and realistic one, as the care of such chronic patients is essentially a nursing and social one. Furthermore, the nurses spend more time with the

patients than anyone else, and are in the best position, by constant close observation, to assess the result of treatment. A general objection to scoring methods based on symptoms is that a situation can arise whereby many trivial symptoms improve while a few important ones remain unaltered or even worsen, the patient consequently achieving a raised overall score, while in fact deteriorating. In none of the trials reported in the literature using such a scoring method was any attempt made to weight scores according to the importance of symptoms.

In a drug trial on patients whose mental state fluctuates, and where the effect of suggestion can be so powerful on both patient and therapist, it is maintained that only the technique of the double-blind controlled trial can yield valid results. A survey of the literature on the effects of chlorpromazine in chronic psychotic groups reveals that of a total of seventeen studies nine are uncontrolled against an inert preparation. Of the remaining eight controlled studies two are confined to the older age group.

Vaughan, Leiberma and Cook (1955) report 70 per cent. improvement with chlorpromazine in doses of 150-200 mg. daily in a group of 103 chronic schizophrenics, the great majority of whom belonged to the catatonic and paranoid subgroups. It is understood that most of these patients relapsed after receiving an identical placebo. As a further study a controlled comparison was made using 48 excited "chronic patients of poor prognosis", 24 treated with chlorpromazine and 24 with a placebo. A significant difference at the 1 per cent. level was revealed in favour of chlorpromazine.

A possible explanation of the difference between the results of this and the present study may lie in the inclusion by Vaughan *et al.* of a very high proportion of catatonic and paranoid schizophrenics. There is general agreement that chlorpromazine helps acute cases more than chronic ones and the tense and aggressive more than the quiet. Furthermore, in no fewer than six papers attention is drawn to the fact that chlorpromazine would appear to benefit the catatonic and paranoid subgroups of schizophrenics rather than the simple and hebephrenic varieties. In the present study only nineteen cases are classified as catatonic and paranoid schizophrenia, but some doubt must be expressed about the adequacy of the classification of the schizophrenic group, many of whom were admitted many years ago when clinical notetaking and classification were less extended than today. With the passage of time many of these cases have deteriorated and it is difficult at this stage to distinguish the different clinical varieties. However, it is of interest that in the present study the drug proved singularly ineffective in a group of twelve chronic paraphrenics.

Five controlled studies are more strictly comparable with the present one, with regard to diagnosis, chronicity, age, etc.:

Elkes and Elkes (1954) carried out a carefully controlled study in 27 chronically over-active patients who were used as their own controls, a dosage of chlorpromazine of 150 mg. daily by mouth being used. It is only necessary here to consider the 13 patients suffering from chronic schizophrenic disorders who, being over-active, correspond with the group of seventy tense patients (groups H and HS) in the present study. Results assessed "blindly" by nursing and medical staff show that when on chlorpromazine as compared with alternating periods on control tablets 3 (23 per cent.) were definitely, and 5 (38.5 per cent.) slightly improved.

These numbers are small and, although much more favourable, a strict comparison cannot be made with the results of the present study, as a different method of comparison is used which does not lend itself to statistical handling.

Shepherd and Watt (1956) carried out a self-controlled study on twenty-four deteriorated schizophrenics. This included a comparison of the effects of 300 mg. daily of chlorpromazine and a placebo with the following results:

	Improved	Unchanged	Worse
On chlorpromazine	15	5	4
On placebo	11	9	4

The results in these small groups do not differ significantly and it is only by comparing the effect, not with the initial state, but with the state before each new drug was given, that a significant effect could be obtained.

Kovitz *et al.* (1955) report a well-controlled study which includes a comparison of chlorpromazine and a placebo in 150 chronic psychotics of whom 127 were chronic schizophrenics. The dosage used was 100-400 mg. daily and up to 600 mg. in a few cases. Fifty-eight per cent. of the patients are reported improved on chlorpromazine as compared with 24 per cent. on the placebo. The comparable figures for the present study for all grades of improvement in 138 patients are: on chlorpromazine 28 per cent. and on placebo 22 per cent. Some possible reasons for this discrepancy occurring in a report from the U.S. are discussed below.

Tenenblatt and Spagno (1957) in a controlled study, also from the U.S. on one hundred paired negro females, mostly schizophrenics, gave doses of 300-900 mg. of chlorpromazine daily for 16-116 days. The evaluation of results was carried out "by several people". The figures for all grades of improvement were: chlorpromazine 76 per cent.; controls 8 per cent.; and the figures for "all degrees of improvement above slight" were: chlorpromazine 40 per cent.; controls 2 per cent. It is of interest that when the catatonic and paranoid schizophrenics are eliminated the figure for improvement on chlorpromazine falls to 30 per cent. The low figure for improvement in the controls differs very greatly from those reported in most other studies on similar groups.

Mitchell (1956) in a carefully planned double-blind controlled experiment on sixty disturbed schizophrenic patients (mostly paranoid) used dosages of 150 mg. of chlorpromazine daily, repeating the experiment with double this dose. It was concluded that statistically there was no change in the symptoms of aggression in this group with the stated dosage of chlorpromazine.

There are two general factors regarding practice in America which merit consideration in any attempt to explain the more favourable reports from that country. Firstly, as Sargent (1956) has pointed out there tends to be a higher level of therapeutic endeavour directed towards the chronic patient in this country, thus leaving less scope for improvement with any further treatment. Details have been given of the previous physical treatments received by the patients in the present study, as it would be interesting to know how all this background activity compares with the position in American hospitals from which reports have been published.

A further possible reason for differences lies in the much higher doses usually reported from the U.S. A majority of British authors have advocated a daily dosage of about 150 mg. daily of chlorpromazine by mouth as being the optimum: Elkes and Elkes (1954); Lomas (1955); Baker (1955); Vaughan *et al.* (1955); Dewhurst (1955). Very few European workers report the use of more than 300 mg. daily.

In a comparable series of reports from the U.S. the following daily doses are mentioned: 200-300 mg. I.M. By mouth: 400-700 mg.; 50-400 mg.; 100-400 mg. "and up to 600 mg. in a few cases"; 300-900 mg.; "300 mg. with a maximum useful dose of 2,000 mg. daily". There is one report of seizures occurring on a dosage of "up to 4,000 mg. daily", and one of fatal hyperpyrexia in a patient who had built up to a dosage of 2,500 mg. on the eighteenth, nineteenth and twentieth days, death occurring on the twenty-first day, in a coma following after fits.

A consideration of all these reports suggests that a co-ordinated programme of research should be carried out on new preparations which are claimed to be of value in abnormal mental states. As is the practice in the M.R.C. trials many hospitals could co-operate using agreed classifications and criteria of severity of illness and of treatment response.

From the results of the present experiment it is concluded that if a sedative agent is to be used at all in chronic psychotic in-patients then barbiturates are preferable to chlorpromazine on grounds of safety and economy. Although Hunter *et al.* (1955) have pointed out that chronic barbiturate intoxication may cause mental symptoms, nevertheless, in controlled dosage the risks are negligible, and with the stock of the drug in the safe keeping of the hospital staff the suicide risk is virtually ruled out; on the other hand chlorpromazine is a toxic drug with the two potentially lethal complications of jaundice and agranulocytosis. The cost of chlorpromazine is six times that of amylobarbitone: the daily cost in the dosage used in this trial being one shilling for the chlorpromazine and twopence for the barbiturate.

Neither drug in the dosage used in this trial, however, was proved statistically to show a therapeutic advantage over the placebo tablet; a trend in their

favour was revealed and it could perhaps be that by increasing the number of cases in the trial a significant result would be achieved. However, it is felt that the failure to reach significance in a trial on 142 patients (including 70 with increased tension) suggests that the overall value of chlorpromazine or barbiturate in the stated dosage in improving the behaviour of chronic psychotic in-patients is very questionable.

SUMMARY

1. In view of the numerous encouraging reports on the use of chlorpromazine in chronic psychotic in-patients, an experiment was set up using 142 patients to compare the results of this therapy with those obtained by a barbiturate, the traditional sedative in these cases, and the results with an inert placebo. The method used was the double-blind comparative method using identical tablets, the patients acting as their own controls.

2. The results fail to support the view that chlorpromazine does not exert its full effect until it has been given for six weeks, such effect as it does have being equally apparent in three weeks.

3. The results failed to show that there is any significant difference in behaviour on any of three preparations (chlorpromazine 150 mg. daily, amylobarbitone 6 gr. daily, or 6 inert tablets daily).

4. In the seventy patients showing increased tension in their pre-treatment state, a trend was shown in favour of both drugs as compared with results of placebo therapy. Considering improving effects only, and excluding all worsening effects, there was still no significant difference between results from the three preparations, although once again a trend in favour of the drugs is seen in the tense group. It is felt that with failure to reveal any significant difference between the responses in a group of this size, the value of barbiturates and chlorpromazine in the stated doses is highly questionable in such a group of chronic psychotic patients.

5. Although the numbers improving on both drugs are almost identical, nevertheless there are indications that some patients showing certain characteristics may show a preference for one drug as against the other. On the whole, however, a given patient will tend to respond in the same way and equally with either drug.

6. The psychological effect of the trial is shown to cause slight degrees of improvement most marked in patients whose initial tension state was neither raised nor lowered from the normal.

7. Both drugs were singularly ineffective in a group of twelve chronic paraphrenics.

8. The findings, although consistent with, do not support the view that one of the virtues of chlorpromazine as compared with barbiturates is a lower incidence of drowsiness. The results failed to support the view that flushing is a side effect of chlorpromazine therapy. On the other hand pallor did appear to be a side effect particularly during the first three weeks of medication. No relationship was discernible, however, between these forms of autonomic response and response to treatment.

9. Possible reasons for differences between these and other reported results are discussed and a plea made for a programme of co-ordinated research using agreed classification and criteria.

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REFERENCES

- BAKER, A. A., *J. Ment. Sci.*, 1955, **101**, 175.
DEWHURST, K., *Irish J. Ment. Sci.*, 1955, **6**/350, 83.
EDWARDS, A. L., *Psychometrika*, 1948, **13**, 185.
ELKES, J., and ELKES, C., *Brit. Med. J.*, 1954, *ii*, 560.
HUNTER, A. R., MERIVALE, W. H. H., and SMITH, A. J., *Lancet*, 1955, *ii*, 1353.
KOVITZ, B., CARTER, J. T., and ADDISON, W. P., *A.M.A. Arch. Neurol. and Psychiat.*, 1955, **74**, 467.

- LOMAS, J., *Brit. Med. J.*, 1955, *i*, 879.
MCNEMAR, Q., *Psychometrika*, 1947, **12**, 153.
MITCHELL, P. H., *J. Ment. Sci.*, 1956, **102**, 151.
SARGANT, W., *Brit. Med. J.*, 1956, *i*, 939.
SARWER-FONER, G. J., and OGLE, W., *Canad. M.A.J.*, 1956, **74**, 526.
SHEPHERD, M., and WATT, D. C., *J. Neurol. Neurosurg. Psychiat.*, 1956, **19**, 232.
TENENBLATT, S. S., and SPAGNO, A., *J. Clin. Expt. Psychopath.*, 1956, **17**, 81.
TEWFIK, G. I., *Lancet*, 1956, *i*, 1179.
TRELLES, J. O., and SAAVEDRA, A., *Rev. Neuro. Psiquiat.*, 1954, 17/2, 125.
VAUGHAN, G. F., LIEBERMAN, D. M., and COOK, L. C., *Lancet*, 1955, *i*, 1083.

SOME IMPLICATIONS OF THE EYSENCK-PRELL STUDY OF "THE INHERITANCE OF NEUROTICISM": A CRITIQUE

By

BERTRAM P. KARON*

Akron Psychological Consulting Center, Akron, Ohio

and

DAVID R. SAUNDERS

Educational Testing Service, Princeton, New Jersey

INTRODUCTION

FEW problems in the whole field of psychopathology are more important than that of aetiology. While it is generally accepted that heredity and environment must interact in some fashion in the causation of most mental disorders, the nature of the roles played by the two classes of determinant is not known. Consequently, any study shedding light on this problem is of primary importance.

In reviewing the evidence on hereditary determination of mental disorders, Kallman (6) cites *only one study* pertaining to the neuroses, as follows:

"Pursuing a 'heuristic approach to neuroticism as a general factor in the conative sphere', Eysenck and Prell were among the few investigators of psychoneurotic reaction patterns (outside the field of criminality) who availed themselves of the opportunities offered by the twin-study method. As a result of their study of 25 one-egg and 25 two-egg pairs, they classified 'the neurotic personality factor' as a biological and largely gene-specific entity, estimating the genetic contributions to this neurotic unit predisposition at 80 per cent." (p. 111).

This study, conducted by Eysenck and Prell (2) and cited by Kallman, deserves even more attention than it has received. This is not to say that it has gone without notice, but rather that most reactions to it seem to amount merely to unqualified acceptances or rejections of the stated results. Such uncritical evaluations are perfectly understandable. On the one hand, the apparent thoroughness and methodological sophistication of the Eysenck-Prell study are unparalleled in this area. On the other hand, the results are so much at variance with general clinical experience that doubts arise in the minds of many psychologists—particularly those who have investigated, in the therapeutic situation, the source and development of neurotic reactions. Not all clinicians are equipped by their level of training in statistics to cope with these doubts; those who are not inclined to maintain an open mind can only resolve their doubts by an act of faith, with probable injustice to one side or the other. Kallman himself appears to take an open-minded position:

"As previously noted, current information about verified inheritance factors is least complete in the vast area of psychoneurotic traits, where representative twin data are most difficult to secure and few genetic questions have been fully answered. Despite the unmistakable existence of variable psychosomatic vulnerabilities, it is even disputed by some schools that genetic phenomena are of any significance in this area" (p. 109).

* This paper was completed while Mr. Karon was a Public Health Service Research Fellow (at Princeton) of the National Institute of Mental Health.

Even though the proponents of heredity do not maintain the equivalence or the necessity of a relationship between hereditary determination and invulnerability to therapy (6), they nevertheless imply we should act *as if* this were so (5); the possible implications of the Eysenck-Prell study are of great practical importance to the practising clinician. Citing Kallman (6) again:

"The questions most frequently brought to a psychiatric-genetic counselling centre have to do with the advisability of having children, or placing a child for adoption, where there is a known history of mental illness, mental defect, or a gross malformation in one or both of the parents or prospective parents or their families . . ." (p. 252).

"To be sure our knowledge of human heredity in relation to psychiatric problems is still incomplete, but this is also true of a multitude of non-genetic influences on human behaviour. Whatever information is available on the genetics of general health and mental disorder is based on well-substantiated evidence which easily withstands categorical denial. In practical application, this evidence can be discussed with psychiatric patients or their families just as tactfully and advantageously as precipitating or perpetuating ecological factors of causation" (p. 262).

However, the clinical practitioner confronted by the Eysenck-Prell findings is in the unfortunate position of having no alternative other than uncritical acceptance or rejection. We use the term unfortunate because we believe neither that the authors' interpretation of their findings should be accepted simply because the statistical smoke screen is too dense to penetrate nor that it should be rejected because it appears to contradict common experience. Rather, we believe the authors' interpretation should be rejected—or, at best, severely limited—primarily on the basis of the authors' own report of their statistical procedures.

Since a methodological analysis of this important study has not heretofore appeared in the literature, and since one is called for by the present state of affairs, it is a necessary purpose of this paper to undertake such an analysis.

THE EYSENCK-PRELL STUDY

The study which we have set out to examine (2) consists essentially of a comparison of a sample of identical twins with a second sample of fraternal twins, in terms of the degree of agreement between members of each twin-pair, using scores on a particular "neuroticism factor" extracted by factor analysis from a particular battery of 17 tests. As much as possible, we will summarize this study in the authors' own words:

"The birth records for five boroughs in South London were searched for all twins of the same sex, born during the period 1935-37 . . . The survey of the birth records yielded the names of 130 pairs of like-sex twins. From these it was possible to locate 68 pairs who were living in the London area, close enough to attend the Psychological Laboratory of the Institute of Psychiatry. The remainder were either living too far away, had died, or could not be located. In no case was parental permission to test refused. The twins were examined as they were located. After examination they were classified as identical or fraternal according to a procedure to be described in the following section" (p. 450).

The twins were classified with respect to zygosity on the basis of the following: close resemblance of ears, teeth, and facial features; iris pigmentation; standing height; presence or absence of mid-digital hair; ability to taste phenylthiocarbamide; scapular shape; blood type.

"On the basis of the above procedure, 20 pairs of twins were classified as definite MZ (monozygotic), 24 pairs as definite DZ (dizygotic), and 6 pairs as doubtful" (p. 452).

Five of the doubtful pairs were later classified as MZ and one pair as DZ, making exactly 25 pairs of each type. (It should be noted that we are

nowhere explicitly told why 18 of the 68 pairs, presumably located and examined, were eliminated from subsequent analyses.)

Each of the subjects had been administered the following battery of 17 tests: (1) Wechsler-Bellevue, Similarities and Digit-Symbol Subtests; (2) Tapping Area; (3) Tapping Speed; (4) Level of Aspiration; (5) Motor Speed; (6) Speed of Decision; (7) Static Ataxia; (8) Body-Sway Suggestibility; (9) Motor Speed Test; (10) Word Dislikes; (11) Brown Personality Inventory (Adapted); (12) M.M.P.I. Lie Scale (Adapted); (13) Flicker Fusion; (14) Autokinetic Movement; (15) Autokinetic Suggestibility; (16) Speed of Writing S's Backwards; (17) Fluency. Since the psychological meaning of these measures is of minor importance in what follows, we refer the interested reader to the original report for more complete definitions of these tests.

The test scores were correlated, using the sample of 100 individuals retained for the analyses. Three centroid factors were extracted from this matrix. A special form of rotation—called "Criterion Analysis"—was employed in order to maximize the similarity of the loadings obtained on a particular rotated factor to a series of independently-derived numbers expressing the ability of each test to distinguish "neurotics" from "normals". Each twin's score on this rotated factor was computed as a measure of his neurotic tendency; any information contained in the other rotated factors was dropped from further consideration.

The procedure used to define a "criterion" of neuroticism is of importance, since it must be considered in arriving at an evaluation of the reported "validity" of the neuroticism scores computed for the 100 twins.

"Twenty-one children born between 1935-37 were selected from out-patients at the Maudsley Child Guidance Clinic. Great care was taken to exclude children with organic complications, or with possible psychotic traits, or who were not definitely considered 'unstable' by the examining psychiatrist. The resulting sample of 21 children approaches as closely as is possible at the present stage of psychiatric knowledge a 'pure' neurotic group, with relatively little mixture of other mental or physical disorders" (p. 453).

Biserial correlations were then computed to determine the ability of each test (except variable 12) to distinguish between these 21 children defined as neurotics and the 100 twins, defined for this purpose as purely normal. These biserial correlations are the "independently-derived numbers" referred to above. The correlation between these numbers and the loadings of the tests on the selected rotated factor is $+0.758$. (It should be noted that this is *not* a validity coefficient in any conventional sense.)

The means and variances of the two twin samples on their computed neuroticism factor scores are given as:

				Mean	Variance
Identical Twins	..	..	..	23.20	91.55
Fraternal Twins	..	..	..	22.96	41.47

"There seems to be little doubt that in our sample identical twins and fraternal twins have almost identical means with respect to neuroticism (23.20 and 22.96 respectively) but that the identical group contains pairs of twins which tend to be more extremely unstable, and more extremely stable, than the fraternal twin group. We cannot explain this finding and must wait for a replication of the experiment before trying to generalize this tendency to all twins" (p. 460).

Eysenck and Prell then proceed with the computation of Holzinger's h^2 ,

"i.e., the extent to which each test variance is determined by heredity . . . factor scores on the neuroticism factor were calculated for each twin and h^2 values calculated for these 'neuroticism scores'.

$$h^2 = \frac{r - fr}{1 - fr} = \frac{(1 - fr) - (1 - r)}{(1 - fr)}$$

where r = intraclass correlation for identical twins and fr = intraclass correlation for fraternal twins, it follows that

$$(1 - r) = \frac{\text{'within' } i \text{ variance}}{\text{'total' } i \text{ variance}} = \frac{13 \cdot 680}{91 \cdot 551} = 0 \cdot 149$$

from which $r = \cdot 851$.

$$\text{Similarly, } (1 - fr) = \frac{\text{'within' } i \text{ variance}}{\text{'total' } i \text{ variance}} = \frac{32 \cdot 480}{41 \cdot 468} = 0 \cdot 783$$

from which $r = \cdot 217$.

It follows that $h^2 = 0 \cdot 810$ " (pp. 460-1).

"The h^2 technique used in this paper gives the per cent. of twin difference variance attributable to nature providing that certain assumptions are met. Two of these assumptions are that nurture influences are the same for both types of twins, and that differences due to nature are uncorrelated with differences due to nurture. It is probable that these assumptions are not completely met, and that consequently our estimate is too high. On the other hand, another assumption, viz., that the variance due to errors of measurement is negligible, is certainly not fulfilled, and in view of the known unreliability of personality tests we must assume that errors of measurement may play a considerable part. This would lead one to believe that the found h^2 would be an underestimate of the true value, and it seems not impossible that this factor may cancel out the two previously mentioned. It is possible, therefore, to argue that the h^2 found is a rough and ready estimate of the contribution which heredity makes to individual differences in neuroticism" (p. 461).

They add in a footnote:

"We have used Holzinger's h^2 statistic in our estimates because no better estimate of the contribution of heredity to the total variance is available. In view of the various assumptions involved in this statistic we do not feel too much confidence in the accuracy, and would lay more stress on the directly observed intra-class correlations for identical and fraternal twins" (p. 461).

Eysenck and Prell conclude on the basis of this study that

"... we have shown that identical twins show a correlation on neuroticism of $\cdot 851$ while fraternal twins show a correlation of only $\cdot 217$. From this it was concluded that individual differences with respect to neuroticism, stability, integration, or whatever we may wish to call this trait or factor, are determined to a very marked extent by heredity, and very much less markedly by environment. This conclusion, of course, applies only in the general type of environment from which all our twins came, and might not be applicable under conditions of more extreme environmental variation, such as may obtain in other cultures" (p. 461).

To what extent is this conclusion justified?

FAULTY REPORTING

As has been noted above, Eysenck and Prell refer to 68 pairs of twins on page 450 of their report, and to 50 pairs on page 452 and throughout the remainder of their report, without informing the reader of the basis for this selection or, indeed, without even mentioning that any reduction in the sample is taking place. Now, we are accustomed to seeing a few cases in some studies discarded because their data are not quite complete, but certainly 18 out of 68—over 25 per cent.—is much too high a proportion to discard without comment, even though it may be justifiable through the resulting saving in computational labour. Also, we are accustomed to seeing efforts made to balance experimental designs, as by using the same number of pairs of identical and fraternal twins, again for statistical convenience; in the present situation this does not appear to have been a reason, since the obvious consequence of this course would have been simply to use 20 pairs definitely of each type, if that were indeed the maximum number of definitely monozygotic pairs. We are furthermore accustomed to seeing cases eliminated when it will result in greater comparability of samples in terms of variables not under investigation which may contaminate results, but we always regard it as essential to state that this has been done and to state the basis and method that has been employed or,

at least, the effects that have, it is hoped, been controlled by doing it.

No such statement appears in the Eysenck-Prell report. Instead, we find on page 469 a report of evidence that the two samples are, in fact, non-comparable in terms of a crucial variable—degree of variability on the neuroticism factor whose sources are being studied. If the greater variance were observed for the *fraternal* twins, this might simply result from the differential effects of environment on the members of the pair. However, the greater variance is observed for the *identical* twins, and can be interpreted as a *lack* of desirable experimental control. Indeed, the variance of the identical twins with respect to neuroticism factor scores is more than twice that of the fraternal twins. In statistical terms, the F-ratio is 2.21, which is statistically significant at the 5 per cent. level even if we make the conservative assumption of only 24 degrees of freedom for each reported variance estimate.

FAULTY COMPUTATION OF h^2

Eysenck and Prell go to a great deal of trouble to specify various assumptions underlying the computation of h^2 , even including a technical appendix by May (10) devoted primarily to such assumptions. Yet nowhere is there any mention of the crucial assumption that the variance of the identical twins is equal to the variance of the fraternal twins, an assumption their data do not happen to meet. It would still be possible to compute h^2 using data from groups of different variance, but *not* by using the simplified formula actually employed by Eysenck and Prell. The importance of the difference between the two formulae for h^2 has been made clear in the literature in papers by McNemar (8, 9) and Holzinger (4) discussing the Newman, Freeman and Holzinger twin study (11) for which the h^2 statistic was originally developed.

The general formula for h^2 is

$$h^2 = \frac{f^{\sigma^2}(1-r) - i^{\sigma^2}(1-r)}{f^{\sigma^2}(1-r)}$$

where f^{σ^2} is the variance for fraternal twins and i^{σ^2} is the variance for identical twins. This reduces to the formula used by Eysenck and Prell only in the special case when $f^{\sigma^2} = i^{\sigma^2}$ (see Holzinger (4, p. 440)). Use of the simpler formula led to no great error in the Newman, Freeman and Holzinger study because their groups were comparable in variance. However, using the appropriate formula for h^2 on the present data *produces an immediate reduction from .810 to .580*.

Eysenck and Prell suggest on grounds other than these unequal variances, that we "lay more stress on the directly observed intraclass correlations", than on h^2 (p. 461). But a direct comparison of these numbers is no more valid than the use of the simplified formula for h^2 , and for the same reason. Just as with h^2 , it is nevertheless possible to obtain estimates of what these numbers might have been for comparable groups (see Holzinger (4, pp. 438-440)). While such estimates are an improvement on the raw intraclass correlations, the necessary assumptions make them distinctly less satisfactory as a solution than obtaining comparable groups in the first place.

The raw and adjusted intraclass correlations turn out as follows:

	r	r
Raw values	.851	.217
Correlations estimated for groups whose variance equals that of the identical twins	.851	.645
Correlations estimated for groups whose variance equals that of the fraternal twins	.671	.217

pr!

It is clear that there is considerably less difference between the corrected correlations than between the raw correlations. It is also clear that the difference is much more impressive when fraternal twins are made the reference group than when identical twins are made the reference group. There is no obvious *a priori* preference for one of these choices over the other, or, indeed, for either of them over some other arbitrary choice. This suggests a general problem to be discussed below, the appropriate range of variation to be studied in such research as this.

EFFECTS ON H^2 OF OTHER STATISTICAL ASSUMPTIONS

There are at least three other assumptions underlying even the more complicated formula for h^2 which are very difficult to satisfy in practice. These assumptions, all of which are mentioned by Eysenck, are:

1. Variance due to errors of measurement is negligible.
2. Differences due to nature and nurture are uncorrelated.
3. Nurture influences are equally similar regardless of the type of twins.

The first of these need not be a source of insurmountable difficulty. Any well-designed study in this area could and should include empirical estimates of the errors of measurement. A formula for h^2 may be readily obtained which takes account of such reliability coefficients. Such an elementary precaution has not been taken in this study.

The second assumption stated just above seems to us a reasonable first approximation, if one is willing to overlook interactive phenomena. The third assumption, however, is more difficult to dismiss; it is never true and will *always* lead to an overestimate of the role of heredity. This follows from the fact that any two individuals between whom the environment, i.e., other people, cannot easily distinguish are apt to be treated much more similarly than are individuals who are readily distinguished.

FAILURE TO ANALYSE THE ENVIRONMENTAL VARIANCE

The variance due to environment may be regarded as consisting of two components: variance which is the same for both twins in a pair, and variance which is different even for twins in the same pair. We may speak of these as "between-family" and "within-family" environmental variance. The "twin difference variance" computed by Eysenck and Prell includes only the within-family components of environmental variance, not the between-family component. We do not feel that sufficient recognition is given this fact by these authors, who conclude:

"... that individual differences with respect to neuroticism, stability, integration, or whatever we may wish to call this trait or factor, are determined to a very marked extent by heredity, and very much less markedly by environment. This conclusion, of course, applies only in the general type of environment from which all our twins came, and might not be applicable under conditions of more extreme environmental variation, such as may obtain in other cultures" (p. 461).

It would have been more accurate if they had stated substantially that . . . their conclusion applies only when the environment is held as constant as is practically feasible, that is, considering only environmental influences which differ for children who are born at the same time, and who live all their lives in the same families; this conclusion is not applicable to conditions of more extreme environmental variation, such as may obtain between siblings of different ages, or between different families, let alone between different cultures.

QUESTIONABLE VALIDITY OF THE NEUROTICISM CRITERION

Several ideas which suggest that the "criterion" of neuroticism used in this study is extremely limited and possibly severely biased require discussion.

First, let us examine the battery of tests. The basis for deciding on this particular battery of 17 tests is nowhere made clear by Eysenck and Prell. We would naturally presume the tests were chosen for a battery from which a neuroticism factor was to be extracted because of an expected close relationship to such a factor. However, such a presumption is contradicted by the following figures, based on reported data:

	Test						r_{bis}^2	h^2
1	..	..	..	..	..	..	.003	.676
2	..	..	..	..	..	..	.060	.269
3	..	..	..	..	..	..	.008	.547
4	..	..	..	..	..	..	.003	.242
5	..	..	..	..	..	..	.046	.528
6	..	..	..	..	..	..	.029	.193
7	..	..	..	..	..	..	.092	.692
8	..	..	..	..	..	..	.033	.701
9	..	..	..	..	..	..	.003	.628
10	..	..	..	..	..	..	.056	.210
11	..	..	..	..	..	..	.004	.145
12	..	..	..	..	..	..	.000	.418
13	..	..	..	..	..	..	—	.627
14	..	..	..	..	..	..	.028	.648
15	..	..	..	..	..	..	.002	.461
16	..	..	..	..	..	..	.004	.423
17	..	..	..	..	..	..	.001	.270

r_{bis}^2 gives the percentage of variance of each test that is due to the criterion dimension; h^2 gives the percentage of variance said to be due to heredity, according to the authors' use of h^2 . These figures provide a striking impression that the tests might just as well have been chosen on the basis of having a high hereditary determination. While we cannot judge from these data whether a better "neuroticism" factor could be extracted from the battery, it is quite clear that *any* factor extracted is likely, because of the nature of the tests, to be heavily weighted toward the side of heredity. *In so far as the factor is imperfectly correlated with a true criterion of neuroticism, the contamination is more than likely to be hereditarily determined variance.* Since the battery was factored without including the criterion variable, only variance which the criterion has in common with *at least two* tests in the battery can be brought into the factor structure.

Nowhere are we given a true validity figure for the neuroticism factor. The correlation of .758 is between the set of biserial validities of the tests and the factor loadings of the tests. If the chosen "neuroticism" factor simply accounted for all the variance *common* to the criterion and the tests (omitting Test 12), this reported correlation would become 1.00, *no matter how small a percentage of the criterion variance this was*. All that we may properly conclude from the reported index is that somewhat less than all of this common variance is accounted for by using the one chosen neuroticism factor.

Using subsequently reported data (3), we have computed r_{bis} between the *factor* scores and neuroticism; the result is .731 and, to us, surprisingly high. However, this means that such factor scores account for only 53 per cent.

of the variance of the criterion dimension and, as noted above, these are relatively likely to be the hereditary components of that variance.

What about the definition of the criterion groups themselves? Certainly, the differences between these groups include any differences inherent in *twins versus non-twins*, as well as differences between normals and neurotics. The former are not insignificant (see Burlingham (1) and Karpman (7)). The latter are further blurred by the fact that the "normal" sample of twins must contain some neurotics; indeed, if it does not, this sample cannot contain variance associable with neuroticism and the whole experimental design collapses.

SOME IMPLICATIONS OF THESE COMMENTS

It is quite clear at this point that the study under discussion suffers from serious shortcomings. Aside from a desire to suggest that too much faith should not be placed in this study, we want to point to some considerations that go beyond any particular study of this kind.

We have already noted in passing that the difference between r and r_r looks more impressive if estimates are made for groups whose range of variation is that of the fraternal twin sample than if using groups like the identical twin sample. Which is "correct"? Is either "correct"? How do we decide what is "correct"?

It is obvious that, if both hereditary and environmental determinants are involved in a phenomenon, the percentage of the variance attributable to each will vary with the range of variation in each that is encompassed in the study. The "solution" has to lie in an agreement to reduce all computations to some arbitrary "normal" degree of variation, which may be even harder to define than to agree on.

However, we feel that such a course is as futile as it is plausible. Such a course is based on a sterile formulation of the real problem. Let us cease asking, "What percentage of the variance is due to this or that?", being blinded by the glittering neatness of the additivity theorems in the analysis of variance texts. Let us ask instead, "What is the nature of the hereditary *and* environmental mechanisms which underlie this or that phenomenon?"

For example, if somatotypes themselves are hereditary, and if children with different somatotypes tend, as a result of their childhood experiences, to learn correspondingly different ways of handling aggression, are these resultant differences hereditary or environmental? What you call them is purely a matter of definition and is of no real importance. What is important is to spell out the mechanism in enough detail so that rigorous empirical tests may be applied that are capable of differentiating between this mechanism and other reasonable alternative mechanisms, and then to carry out such tests.

To cite another example, it is far less valuable to know that sickle-cell anaemia is "100 per cent. hereditary" than to know it can be associated with a single recessive gene. As a result of this gene, defective red blood cells are formed, and the defects in molecular structure which cause sickling can be described in some detail. The homozygous carrier of the gene dies before the age of reproduction. In the heterozygous carrier of the gene, half the red cells are defective while the other half are normal; the presence of the sickle-cells actually confers a relative immunity to malaria, accounting for the prevalence of the sickle-cell disease in certain areas heavily infested with malaria. Again, it seems meaningless to try to divide responsibility for this situation between heredity and environment.

Only our efforts to understand determining mechanisms seem likely to produce successful attempts to remedy or prevent situations of any sort, including the neuroses. Such understanding of the underlying order in the universe is the proper goal of scientific investigation.

SUMMARY

The Eysenck-Prell study discussed above serves to illustrate the principle that methodological complexity is no substitute for methodological adequacy. The Eysenck-Prell conclusions are far from justified, for the following reasons:

1. There is a disparity in criterion variance between the samples of identical and fraternal twins, with a resulting miscalculation of h^2 and lack of comparability between r and r_r , the intraclass correlations of the two samples.

2. Other uncontrolled assumptions of the h^2 technique lead us to regard it as only a very rough estimate.

3. The environmental variance isolated in the study includes *only* the environmental influences that are different for the two members of a pair of twins living together, omitting other important sources of environmental variance.

4. The validity of the neuroticism criterion itself is dubious; it may be seen to be contaminated by twin vs. non-twin variance, and biased in favour of hereditary components through the selection of tests.

It is notable that all the previously unrecognized sources of difficulty which we have been able to discern in this study have led Eysenck and Prell to inflate the role of heredity. Our own conclusion would be that, under conditions where much if not most environmental variance is held constant, only about 30 per cent. of a crude neuroticism criterion may be attributed to hereditary determinants.

However, we do not think that *any* number such as this is more than a red herring as a contribution to scientific knowledge. So long as we know that heredity and environment are both involved in a given phenomenon, our most fruitful strategy is to study the mechanisms of their *interaction*.

REFERENCES

1. BURLINGHAM, DOROTHY T., *Twins*, 1952. New York: International Universities Press.
2. EYSENCK, H. J., and PRELL, D. B., "The inheritance of neuroticism. An experimental study", *J. Ment. Sci.*, 1951, **97**, 441-465.
3. *Idem*, "A note on the differentiation of normal and neurotic children", *J. Clin. Psychol.*, 1952, **8**, 202-4.
4. HOLZINGER, K. J., Reply to special review of "Twins", *Psychol. Bull.*, 1938, **35**, 436-444.
5. KALLMAN, F. J., *The Genetics of Schizophrenia*, 1938. New York: Augustin, pp. 2-3.
6. *Idem*, *Heredity in Health and Mental Disorder*, 1953. New York: Morton.
7. KARPMAN, B., "Psychodynamics in fraternal twinship reactions", *Psychoanal. Rev.*, 1953, **40**, 243-267.
8. MCNEMAR, Q., Special review: Newman, Freeman and Holzinger's *Twins: A Study of Heredity and Environment*, *Psychol. Bull.*, 1938, **35**, 237-249.
9. *Idem*, Rejoinder to Holzinger's reply to special review of "Twins", *Psychol. Bull.*, 1938, **35**, 552-554.
10. MAY, JOAN, "The assumptions underlying Holzinger's h^2 statistic", *J. Ment. Sci.*, 1951, **97**, 466-467.
11. NEWMAN, J. H., FREEMAN, F. N., and HOLZINGER, K. J., *Twins: A Study of Heredity and Environment*, 1937. Chicago: University of Chicago Press.

WEYBURN ASSESSMENT SCALE*

By

D. B. BLEWETT, Ph.D.

Supervising Psychologist

Psychiatric Services Branch, Department of Public Health, Saskatchewan, Canada

and

W. B. STEFANIUK, B.A.

Research Psychologist Associate

Saskatchewan Committee for Schizophrenia Research

SOME time ago the Department of National Health and Welfare of the Government of Canada became interested in the therapeutic effects of a preparation of nucleotides in the treatment of mental illness. It was decided that the research group from the Saskatchewan Hospital, Weyburn, should assess the usefulness of the preparation in the treatment of chronic schizophrenia.

In this study, as in any attempt to evaluate the effect of a therapeutic procedure, one of the principal difficulties to be overcome was that of establishing a criterion against which change in the degree of psychiatric illness of the subjects could be measured.

One of the most common methods of assessing change after therapy is to determine the number of individuals who are discharged from hospital subsequent to the treatment under investigation. This method is of limited value since the criterion is too gross. Clear-cut changes may occur with regard to such variables as the ease of nursing care or the acting out of hostile impulses, but such changes cannot be assessed when discharge is the sole criterion of improvement.

A second method commonly used to measure change is the classification of such change as "worse", "much worse", "improved", "much improved", and "recovered". In his criticism of this method Malamud (10) has pointed out that it fails to indicate the extent to which the patient's present mental status differs from his pre-morbid condition, and that the degree of impairment is measured against a theoretical average, which involves us in the time-worn problem of describing "normality" when applied to human behaviour.

For the purpose of this study it was felt that the criterion measure should be in the form of a rating scale which would, as far as possible, yield objective, valid and reliable assessments of the degree of illness, that we should aim at the inclusion of all the rateable features of chronic schizophrenia. In short, we should attempt to develop a scale which would be essentially an operational definition of that condition.

A review of the literature dealing with the objective assessment of therapeutic change in hospitalized psychiatric patients failed to offer any measuring

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This article has been approved by the Saskatchewan Committee for Schizophrenia Research.

instrument which seemed to meet the conditions of the setting in which the study was to be carried out. Rating scales such as those of Plant (1922), Wilcox (1942), Cohen *et al.* (1944), Scherer (1951), Lorr (1953) and Baker and Thorpe (1956) were devised chiefly for assessment of patients by the nursing staff, whereas in the present study, ratings were to be made by trained psychiatrists. While Wittman (1948) and Malamud and Sands (1947) had produced scales which were devised for use by psychiatrists, it was felt that these scales, which had been developed to provide assessments across the wide spectrum of schizophrenia, did not deal in sufficient detail with the specific area of chronic schizophrenia. Further, the methods of assessment reviewed generally involved pre-requisites which could not be met in our hospital. Despite the fact that we drew heavily on Malamud's experience, and became indebted to him for his discussion on the theory and application of rating scales, we found our facilities to be inadequate for the use of his form, which utilizes as its base line the pre-morbid level of personality and measures change as "worse" or "better" from this level. His method is an excellent one if two conditions can be met—(a) the case histories available and interviews conducted and reviewed are extremely full and reliable, and (b) a full and supplementary staff is available to carry out the task. In our case we found gross deficiencies in our case records, especially in the files of those patients who had been hospitalized many years. We did not have the staff to meet the requirements of the method, particularly as regards social workers. We were thus confronted with the need for an objective, reliable method of rating which would permit relatively sensitive assessment of all possible facets of chronic hospitalized schizophrenia across the range of severity of illness and which could be utilized with a minimum of staff.

THE INITIAL FORM

A tentative assessment form was prepared. This form included seven broad behavioural categories (appearance, behaviour, thought, perception, mood, memory and insight). These categories were broken down into 49 variables or items (for instance, appearance was assessed on the basis of the nine variables or items: cleanliness of dress, shoes, cleanliness of nails, cleanliness of hands and face, hair, general order of dress, shaving, wetness and smell). Each of these items was scored on a three-point scale. Since in this study we were concerned with change occurring in a specific experimental situation, we were able to meet Malamud's criticism against measuring from the theoretical average by establishing as a base line the level of behaviour exhibited by the patient immediately prior to treatment. The use of this method permits measurement in any direction of changes which take place during a period of therapy.

A meeting of all the doctors in the hospital was called and the separate scoring items were discussed. The search for general agreement proved to be interesting but arduous, for the medical staff represented many frequently divergent points of view.

THE PILOT STUDY

The initial scale was used in a small pilot project aimed at determining its adequacy. In the pilot study, five patients were rated by each of three psychiatrists. The project was carried out in one day with as little time as possible between interviews.

RESULTS OF THE PILOT STUDY

The results of this trial provided the material upon which the first revision was based. The scale was assessed in terms of the extent to which the raters found it to be clinically adequate and in terms of the level of inter-rater agreement. Each item and each behavioural category was examined for inter-rater reliability to permit modification or rejection of those items in which agreement proved to be low.

Changes and additions were made to clarify the definitions and lessen the likelihood of subjective variation in interpretation. Instructions were set down as a guide to the assessor, particularly during his first few attempts at using the scale. The raters felt that it was not until after some three or four assessments that they could work without the guide. The problem of prompting was a very important one. Where prompting was considered permissible, the number of times a subject might be prompted for each scoring item was specified and the specific method of prompting was set down. Each scoring point was defined by an item description in order to minimize subjectivity, and, to permit finer differentiation, the system was extended to a five-point scale.

W.A.S. FIRST REVISION

The revised scale contained the same seven behavioural categories as did the initial form. In this revision eleven items were dropped from the scale. For eight of these, no agreement as to interpretation could be reached and scoring differed markedly between raters. Three further items were found to duplicate material covered elsewhere in the scale.

The average time for the completion of the assessment was approximately twelve minutes. Various scoring items were re-arranged such that they follow the sequence of a "model" interview, which facilitated scoring and shortened interview time.

THE NUCLEOTIDE STUDY

It was in this revised form that the scale was used in the nucleotide study, which was in fact its *raison d'être*. In this project the revised scale was administered independently on three occasions by each of three psychiatrists to a group of forty male chronic schizophrenic patients, all of whom had been hospitalized for over five years.

Aside from the assessment of the therapeutic effects of the nucleotide preparation, which has been reported elsewhere (9) the data were further examined with two objects in view.

1. We sought to assess statistically the reliability and validity of the rating scale and to determine, if possible, those general areas or behavioural categories and those specific items in which the application of a rating method seemed appropriate. In other words, we hoped that the statistical analysis would provide us with data basic to further revision and improvement of the rating scale.
2. We were seeking to obtain further understanding of the structure of chronic hospitalized schizophrenia through examination of the factorial structure underlying the ratings, which constitute an operational definition of that condition.

ANALYSIS OF THE DATA FROM THE NUCLEOTIDE STUDY

It is usual to assess the validity of a scale designed to measure degrees of a given illness by initially demonstrating its ability to differentiate between the "target" syndrome group and other populations. The nature of this scale limits its use in the present form almost exclusively to intensive measurement of variation within the chronic schizophrenia category and largely precludes any extensive study involving comparisons with other conditions.

This fact appears to pose a problem in terms of validation, since we cannot demonstrate that we are measuring chronic schizophrenia but must make that assumption. However, we believe this assumption to be justified since the criterion against which validity must ultimately be assessed in studies of psychopathological conditions is psychiatric judgment and diagnosis and it is specifically those behavioural phenomena upon which the diagnosis of chronic schizophrenia is established that have been incorporated in this assessment scale.

Rather, the problem becomes one of internal validation—the determination of whether all the behavioural categories sampled are measures of the same process (e.g., whether they inter-correlate positively). This phase of the analysis was carried out in Ottawa by Dr. J. Donnelly (1953) of the Dominion Bureau of Statistics. The results are summarized in Table I.

TABLE I
Inter-correlation Coefficients of Behaviour Category "Factor" Scores

	Hallucin- ation	Behaviour	Thought	Memory	Appear- ance	Insight	Mood
Hallucination	—	—·170	—·014	—·04	·002	—·022	·040
Behaviour ..	—·170	—	·868	·850	·688	·684	·335
Thought ..	·014	·868	—	·834	·667	·650	·301
Memory ..	·040	·850	·834	—	·684	·658	·258
Appearance	·002	·688	·667	·684	—	·336	·364
Insight ..	—·022	·684	·650	·658	·336	—	·312
Mood ..	·040	·335	·301	·258	·364	·312	—

The remainder of the statistical analysis was carried out by the authors.

The nature of the data is such that the principal concern must be with the question of reliability. The data lend themselves readily to such evaluation and the reliability of the obtained ratings has been assessed in the following way:

1. Inter-rater coefficients were computed for each of the three ratings to determine how well raters agreed with each other on different occasions. These results are summarized in Table II.

TABLE II
Inter-rater Consistency Coefficients for Three Ratings by Items

Rater		X					Y			
		1st	2nd	3rd	Total		1st	2nd	3rd	Total
Y	..	..	·70	·76	·80	·73	—	—	—	—
Z	..	..	·73	·71	·72	·72	·74	·77	·79	·77

2. Inter-rater coefficients were computed for each of the behavioural categories to assess how well the raters agreed in their ratings of each type of behaviour. These results are summarized in Table III.

TABLE III

Inter-rater Consistency Coefficients for Behaviour Scores on First Rating

				X		Y
				Z	Y	Z
Appearance	..	..	..	.733	.764	.771
Behaviour	..	..	..	.890	.801	.910
Mood	..	..	..	.047	— .206	.247
Thought	..	..	..	.768	.724	.801
Memory	..	..	..	.969	.953	.985
Hallucinations	..	..	..	.351	.333	.260
Insight	..	..	..	.711	.610	.599

3. An item analysis was carried out in which the scores for each item subsumed in a behavioural category were correlated with the total scores for that category. These results are summarized in Table IV.

TABLE IV

Behaviour	Appearance		Thought	Memory		Mood	
Posture	.84	Nails	.71	Sentence	Ward	.91	Appropriate-
Rapport	.79	Clean (hands		Structure	Birth and Age	.91	ness
Obedience	.79	and face)	.70	Associative	Hospital	.87	Dominant
Gait	.77	Shoes	.65	Process	Province	.86	Mood Tempo
Salutation	.76	Hair	.65	Thought	Name	.83	
Departure	.73	Order	.62	Blocking	Duration of Stay	.82	
Handshake	.73	Cleanliness		Speed (R.V.)	Date	.78	
Motor Speed	.72	of Dress	.60	Thought			
Mannerisms	.61	Shaving	.52	Production		.84	
Degraded	.32	Smell	.34	Neologisms		.15	
		Wetness	.32	Delusions		.01	

Insight and Perception are not included as they represent single items.

4. Test-retest reliability coefficients were calculated to determine the degree of consistency in the obtained ratings by comparing each rater's initial assessments with the ratings he assigned the same subjects after a sixty-day interval. These results are summarized in Table V.

TABLE V

Test-Retest Reliability Coefficients After 60 Day Period for 20 Controls

						X	Y	Z
Appearance	..	..	..	..	..	.51	.67	.72
Behaviour	..	..	..	..	..	.80	.92	.89
Mood	..	..	..	..	..	.08	.59	.32
Thought	..	..	..	..	..	.43	.85	.78
Memory	..	..	..	..	..	.92	.89	.89
Hallucinations	..	..	..	..	..	.16	.71	.34
Insight	..	..	..	..	..	.70	.71	.54

DISCUSSION OF RESULTS REGARDING RELIABILITY AND VALIDITY

Donnelly's results (see Table I) indicate that ratings of individual differences in mood do not accord well with variation found in other behavioural categories. While to a certain extent this lack of relationship would result from instability of mood in the individuals in the sample, the correlations between mood and the other categories were so low that an analysis of the mood items was undertaken to find the source of the unreliability.

An examination of the distribution of item scores indicated that in the case of mood appropriateness and mood tempo, five-point scaling was a spurious refinement since the distributions were basically dichotomous. The distributions of ratings of dominant mood did not differ from the normal. Despite the distributions involved, inter-item correlations were calculated to obtain a rough estimate of the relationship between the items. This analysis showed that the grouping of the items mood appropriateness, dominant mood and mood tempo in the same category was not justified. There was a low negative relationship between dominant mood and mood appropriateness, $-.18$; and a low negative relationship between dominant mood and mood tempo, $-.26$. The relationship between mood tempo and mood appropriateness was low, but positive, $.41$. The combination of these heterogeneous variables contributed heavily to the extreme unreliability of the mood category total score.

In view of the non-normal distribution of mood appropriateness and mood tempo, these variables were not further utilized in their original form. The latter was used in combination with other variables in a manner described in detail at a later point in the discussion.

Attention was therefore centred upon the variable dominant mood. Inter-rater coefficients calculated for this item proved to be higher ($.52$; $.62$; $.35$) than those obtained for the mood category as a whole and presented in Table III ($.05$; $-.12$; $.25$). Test-retest reliability coefficients, with a sixty-day interval, were $.69$; $.19$; $.59$; whereas for the original mood category scores they had been as shown in Table V, $.32$; $.08$; $.59$. These reliability coefficients leave much to be desired. It would appear, however, that this shortcoming is but a reflection of the difficulties encountered in any attempt at assessment in the general area of mood. Not only instability of mood in the rated individuals but also variations in the level of rapport established by the rating psychiatrists contribute to the difficulty of obtaining reliable evaluations.

Of greater interest is the fact that the presence or absence of hallucinations appears to be related to variations in the ratings of behaviour, thought, memory, appearance, insight or mood. This apparently independent status of hallucinations leads to interesting speculation regarding the production of hallucinations in normals through stimulus deprivation or through administration of psychotomimetic drugs and hints at a rationale for the selective effects of chlorpromazine, which reduces anxiety but does not affect the tendency to hallucinate. However, unreliability of measurement, particularly as regards mood, could produce this apparent lack of relationship with other aspects of the chronic schizophrenic syndrome. This point will be further considered in the light of the results of a factorial analysis of the data.

In summarizing the data on reliability it seems worth while to comment on the fact that the inter-rater coefficients as presented in Table II range between $.70$ and $.80$. While one would have hoped for greater inter-rater agreement, it should be remembered that these data include ratings of mood and perception, which would effect a general decline in coefficients. In part, too, these borogove-like coefficients are a function of differences in the clinical approach and the general frame of reference of the three participating psychiatrists. These differences, and the degree to which they are affecting inter-rater reliability, become apparent when one refers to Table III. From these data it becomes obvious that mood should not be rated in the terms utilized in this study and the results of the analysis of the items in the mood category suggest that only the variable dominant mood should be retained in its present form.

Table IV indicates that certain of the items are unrelated to the other aspects incorporated in the same behavioural category—a situation which would tend to reduce category reliability. This is particularly true of the items dealing with neologisms and with delusions and ideas of reference. These apparently bear no relationship to the other items with which they have been classified. The adequacy of the present item classifications will be further discussed in the light of the results of the factorial study.

The material on test-retest reliability which is presented in Table V is based on twenty of the forty cases only—those comprising the control group who received placebo rather than the experimental medication. The experimental group were excluded in order to rule out individual differences in response to medication. The coefficients show that the raters vary in their consistency of assessment. Here again the categories of mood and hallucinations are the least consistent.

Each of the 37 items was examined to ensure that between rater variation was not an important factor. The item appropriateness of mood was rejected on this basis.

The distributions of each of the remaining thirty-six items were examined. In the case of ten items the results indicated that ratings on a five-point scale were unrealistic. These items—order, smell, wetness, insight, hallucinations, degraded, neologisms, obedience, motor speed and mood tempo—yielded “j”-shaped distributions. Rather than leave this data out as not being amenable to parametric methods, it was decided to combine the items such that the distribution would tend to normalize.

The method of combination involved dichotomizing each item distribution at the score closest to the median of the distribution. A score of one was given to a rating above the median and a score of zero was given to a rating below the median. Sub-median scores for each of the items were found to be: Order 1; Smell 1; Degraded 1; Wetness 1; Insight 1, 2, 3 and 4; Hallucination 1 and 2; Neologisms 1; Obedience 1; Motor Speed 1 and 2; Mood Tempo 1; which were accordingly given a zero score.

A table of co-occurrence was drawn up to show for each case in the sample the pattern of one and zero scores. It was found that seven variables formed one cluster or group while the remaining variables tended to form a second independent cluster. These sets of items were labelled Group A and Group B. Group A includes the items Order, Smell, Wetness, Degraded, Obedience, Speed (motor), and Insight. Group B includes the items Mood Tempo, Neologisms and Hallucinations. Scores were derived for each individual in the sample by simply counting the number of scores that he obtained (e.g., the number of higher than median ratings) in each of Group A and Group B. These groups were found to be slightly negatively related ($r = -0.20$). For Group A the distribution did not differ from the normal. For Group B, however, which comprises only three items, the distribution was positively skewed and could not be normalized. In consequence, correlations involving Group B can be expected to be relatively low. However, this data was of such interest that it was retained in the study.

RATIONALE FOR FACTOR ANALYSIS

In this analysis we were seeking to obtain further understanding of the structure of chronic hospitalized schizophrenia through studying the factorial structure underlying the ratings. We had, in the course of building the scale, been involved in much discussion as to whether we were dealing with a unitary

disorder in which the symptom sub-groups (catatonic, paranoid, etc.) were differing expressions of the same disease process or whether we were dealing with a group of separate disorders having certain symptoms in common. A preliminary decision was made to place emphasis on category, rather than total, scores. This procedure implies the acceptance of the latter rationale; that is, schizophrenia is a generic term which encompasses a group of disease entities which have certain symptoms in common. This rationale may be tested on the basis of the inference that when the ratings of schizophrenia are factor analysed the obtained factor structure would reflect the underlying pattern of separate diseases. Accordingly, we would expect, if clinical classification be valid, to obtain a paranoid, a simple, a hebephrenic and a catatonic factor. This theoretical position can be formalized into the following hypothesis:

When a factor analysis is carried out on ratings derived from the Weyburn Assessment Scale, the factors extracted should present patterns of factor loadings such that they may readily be identified as descriptive of the basic clinical sub-divisions of schizophrenia.

At this point in the discussion many readers will take issue with the authors. Disagreement will centre around two points. Some will take the point of view that whatever the nature of the results they will be without practical importance, while others will feel that the method of factor analysis is unsuited to the problem and to the investigation of the hypothesis under examination.

Of those who see the results as unimportant, some may hold the view that regardless of the outcome of any factor analysis, the nosology derived through decades of clinical experience and observation is unlikely to be modified. Others will point out that in clinical practice there is a trend to regard diagnostic considerations as secondary to those of treatment, since each case, being unique, demands an individualized therapeutic approach—a situation which renders any procedure of fitting the specific case into a broad diagnostic category of little value.

While these arguments are expressive of an extremely practical outlook, it remains evident that while the problem of the nature of schizophrenia remains unsolved, clinical methodology in this area cannot be based on scientific understanding.

Eysenck (1953) has pointed out that, in general, factor analysis is the method best suited to problems of classification—an observation particularly cogent to our present study in which we are seeking to clarify the structure of one specific area of schizophrenia. However, while there may be general agreement on this point, we are open to challenge with regard to the method of factor analysis used in this investigation. Cattell (1956), we believe, summarizes the case against our procedure when he suggests that the general schizophrenia factor should have been obtained as a second order factor, thus leaving the factor more determined.

Although the method that Cattell proposes has a great deal to recommend it, in the authors' view, there are certain considerations which contra-indicate its use.

Clinical experience with diagnostic classifications will tend to exert an influence upon ratings made by psychiatrically trained people. The presence of one symptom of a symptom complex will enhance the probability that the other symptoms of that particular syndrome will be observed in the same individual; not only because such symptoms apparently tend to co-occur, but also because the psychiatrist expects and looks for them to do so. This is particularly evident in those studies in which the variables involved are

symptoms. By using actual aspects of behaviour, rather than symptoms as such, we have attempted to minimize this difficulty.

Guertin (1951) refers to this halo effect, yet proceeds to enhance its reflection in the data by using Thurstone's multiple group centroid method of factor analysis, "with only the symptoms showing tightest clusterings determining the centroids". When this method is used, the probability exists, even when the ratings are not on a specific symptom basis, that the text-book is being transposed through the data into the obtained factor structure, and that the outcome has been pre-determined by the method employed.

If what we call schizophrenia is, in fact, a group of diseases, the authors believe that the only way to isolate these differences is to remove the communality which will be accounted for by a general factor and to examine the nature of the remaining group factors, for in these should lie the key to the nature of the subsumed disease entities. If such entities exist within the rather narrow confines of the study, that is, within the chronic hospitalized schizophrenia group, the nature of the data should enhance the probability of their being readily demonstrated. If they are distinctive disorders their differences should be indicated by a lack of correlation in some areas and clusters of correlation elsewhere. This would tend to maximize the group factors as opposed to the general factor. In this situation there is a high level of inter-relationship between the variables as we are dealing with the interior of the syndrome. Thus there is for each factor a minimum of material in the hyperplane. This relative density of the matrix will enhance the tendency for intra-syndrome group factors to appear. It therefore follows that if the procedure of breaking chronic schizophrenia down into various clinical sub-types has any validity, these classifications should be readily identifiable in the factor structure and should be clearly demonstrated since they can be expected to account for a relatively large amount of the variance.

VARIABLES INCLUDED FOR ANALYSIS

The memory scale items were highly inter-correlated as shown in Table VI. Because of this high degree of inter-relationship, it was deemed inadvisable

TABLE VI
Intercorrelations of Memory Items

	Own Name	Hospital	Ward	Date	Province	Duration of Stay
Own Name	—					
Hospital	.70	—				
Ward	.79	.81	—			
Date	.75	.71	.69	—		
Province	.70	.70	.79	.66	—	
Duration of Stay ..	.56	.64	.64	.80	.60	—
Birthday and Age ..	.68	.75	.80	.74	.77	.77

to include them as a group. A preliminary factorization of the memory items was therefore carried out to determine the extent to which they were univariate. The Thurstone centroid method of analysis as demonstrated in Thurstone (1947) was used. The first factor extracted accounted for 73 per cent. of the variance. The loadings obtained were: Name .83; Hospital .86; Ward .89; Date .86; Province .84; Duration of Stay .81; Birthday and Age .89.

A second factor was extracted but accounted for only .5 per cent. of the

TABLE VII

	Hair	Shaving	Clean (Hands and Face)	Nails	Shoes	Clean (Dress)	Salutation	Handshake	Posture	Mannerisms	Departure	Rapport	Gait	Dominant Mood	Speed (R.V.)	Sentence Structure	Delusions	Associative Process	Thought Production	Thought Blocking	Ward	Group A	Group B
Hair	.39																						
Shaving	.35	.19																					
Clean(handsandface)	.37	.17	.97																				
Nails	.44	.19	.36	.34																			
Shoes	.40	.18	.35	.33	.33																		
Cleanliness (dress)	.41	.23	.42	.53	.39	.23																	
Salutation	.33	.20	.24	.02	.36	.09	.62																
Handshake	.44	.35	.18	.16	.48	.25	.51																
Posture	.44	.35	.18	.16	.48	.25	.51																
Mannerisms	.22	.18	.10	.12	.27	.04	.14																
Departure	.32	.25	.23	.12	.27	.15	.56																
Rapport	.46	.19	.26	.24	.42	.20	.55	.48															
Gait	.50	.25	.31	.29	.52	.32	.44	.47	.60	.20	.51	.55	.45										
Dominant mood	.45	.12	.19	.17	.32	.19	.39	.39	.41	.07	.24	.24	.46	.43									
Speed (R.V.)	.25	.21	.27	.24	.45	.23	.53	.49	.60	.23	.50	.70	.56	.27	.72								
Sentence Structure	.49	.24	.34	.33	.42	.24	.48	.33	.54	.34	.43	.72	.51	.37	.24	.20							
Delusions	.17	.08	.11	.06	.16	.15	.14	.22	.21	.01	.14	.26	.28	.27	.24	.20							
Associative Processes	.44	.21	.32	.27	.33	.18	.44	.36	.48	.39	.38	.61	.47	.31	.65	.73	.06						
Thought Production	.40	.18	.25	.22	.37	.22	.49	.45	.59	.31	.45	.77	.55	.40	.81	.71	.27	.66					
Thought Blocking	.49	.23	.30	.26	.25	.20	.53	.47	.54	.29	.53	.77	.55	.42	.68	.67	.18	.72	.79				
Ward	.38	.16	.34	.27	.39	.23	.50	.46	.47	.28	.41	.60	.45	.32	.68	.67	.17	.62	.67				
Group A	.51	.29	.38	.29	.55	.34	.58	.49	.38	.27	.48	.67	.65	.44	.66	.62	.42	.55	.57	.64			
Group B	.08	.09	.05	.09	.19	.20	.24	.20	.25	.11	.14	.18	.25	.24	.26	.10	.19	.04	.19	.24			

variance. The memory items thus seemed clearly univariate and the item Ward was included in further calculations as representing the memory variable since it had the highest first centroid loading.

Twenty-three variables were included in the final matrix. Of these, twenty-one were included in their original form. The other two variables represent the total scores for each individual of the items which were dichotomized and combined to make Groups A and B.

The matrix of intercorrelations was found between the variables or scale items and is presented in Table VII.

RESULTS OF THE FACTOR ANALYSIS

Six factors were extracted leaving insignificant residuals. The centroid loadings on these factors are presented in Table VIII.

TABLE VIII

Centroid Loadings	1	2	3	4	5	6
1. Hair	.62	.21	-.12	.17	-.05	.14
2. Shaving	.35	.17	-.11	.25	.04	.23
3. Clean (Hands and Face) ..	.48	.45	-.26	-.25	.25	-.05
4. Nails	.43	.51	-.24	-.39	.15	.08
5. Shoes	.60	.18	-.19	.14	-.23	-.13
6. Cleanliness (Dress) ..	.42	.32	-.35	-.21	.03	.10
7. Salutation	.67	-.19	-.09	.23	.18	-.08
8. Handshake	.60	-.33	-.16	.26	.25	-.20
9. Posture	.70	-.18	-.11	.18	-.13	.26
10. Mannerisms	.31	.25	.28	.18	-.18	-.07
11. Departure	.59	-.25	-.07	.23	.31	.05
12. Rapport	.81	-.18	.21	-.07	-.09	.13
13. Gait	.74	-.05	-.15	.13	-.16	.12
14. Dominant Mood	.55	-.25	-.05	-.13	-.15	.06
15. Speed (R.V.)	.82	-.15	.25	-.05	.06	.02
16. Sentence Structure ..	.79	.10	.34	-.04	-.06	.06
17. Delusions	-.32	.27	.20	.29	.22	.11
18. Associative Process ..	.71	.15	.43	.09	.07	.02
19. Thought Production ..	.80	-.18	.29	-.09	.06	.17
20. Thought Blocking ..	.81	-.16	.33	-.06	.16	.15
21. Ward	.74	.04	.27	-.03	.11	-.22
22. Group A	.79	.09	.03	.04	.04	.07
23. Group B	-.28	.32	.26	.31	.12	.21
Percentage of Variance ..	40	6	5	4	2	

Simple structure was obtained after nine rotations. The rotated factor loadings are presented in Table IX.

DISCUSSION OF RESULTS

The first factor, which accounted for some 40 per cent. of the variance, is clearly a general factor upon which all variables are relatively highly loaded. In the case of delusions and Group B (viz. mood tempo; neologisms, obscenities and puns, and hallucinations) the loadings are negative. The inference might be drawn from these results that in so far as delusions and hallucinations may be more numerous and more intense in acute schizophrenia than in chronic schizophrenia, this particular variable may work in a reverse direction to the other measures descriptive of the chronic syndrome. Some support for this view may be found in the work of Miller and his associates (1953) who found in evaluating progress in patients suffering from chronic schizophrenia that a group in treatment who show less disturbance in behaviour and improved rapport show more hallucinations than a control group. Possibly an alternative

TABLE IX

Rotated Factor Loadings	1a	2c	3c	4d	5d	6c
1. Hair	·60	·20	—·07	·16	—·12	·22
2. Shaving	·34	·30	·04	·22	—·08	·15
3. Clean (Hands and Face) ..	·37	·01	·04	—·15	—·13	·67
4. Nails	·30	—·02	—·09	—·08	—·01	·76
5. Shoes	·59	·01	—·19	—·13	—·32	—·08
6. Cleanliness (Dress) ..	·34	—·05	—·05	·11	—·10	·55
7. Salutation	·69	·03	·30	—·01	—·13	—·04
8. Handshake	·63	—·07	·45	—·03	—·21	—·13
9. Posture	·73	·04	·03	·26	·05	—·03
10. Mannerisms	·32	·28	—·31	—·17	—·08	—·06
11. Departure	·61	·09	·43	·05	·00	—·05
12. Rapport	·83	—·03	—·07	—·03	·26	—·05
13. Gait	·75	·00	—·05	·24	—·08	·07
14. Dominant Mood	·57	—·25	—·02	·11	·13	—·02
15. Speed (R.V.)	·83	·01	·04	—·17	·21	—·03
16. Sentence Structure ..	·78	·16	—·19	—·20	·19	·07
17. Delusions	—·33	·49	·07	—·09	—·02	—·02
18. Associative Process ..	·70	·31	—·11	—·30	·16	·01
19. Thought Production ..	·81	·05	·03	—·09	·36	—·02
20. Thought Blocking ..	·82	·10	·11	—·17	·37	·09
21. Ward	·73	·05	—·01	—·39	·01	·05
22. Group A	·77	·12	—·01	—·03	·02	·19
23. Group B	—·28	·56	—·06	—·02	·03	—·03
Percentage of Variance ..	39	4	3	3	3	2

explanation would be that as patients develop better contact with staff members, the staff members become more readily aware of the patient's hallucinatory behaviour or delusional thinking—symptoms which the patient, as he improves, is more likely to discuss. Another possibility may be that though we have a syndrome which shows a high level of internal consistency, as demonstrated by the degree of inter-correlation between scale items and by the relatively heavy saturation of every item with the general factor, we may not be measuring chronic schizophrenia as such but the deterioration effects of chronic mental illness and long-term hospitalization which may not be specific to schizophrenia.

If this were the case, the fact that hallucinations and delusional thinking correlate negatively with the rest of the syndrome could suggest that the chronic pattern as operationally defined in this scale may not be a function of schizophrenia. Rather, it might be related to the level of hospital care—to a deteriorative process resulting from living for prolonged periods of time in a “back ward” environment.

Unfortunately, the question of specificity of the results of this investigation to schizophrenia is outside the scope of the study. It cannot be answered from the collected data. However, with regard to the problem under examination, it can be stated to the extent that the ratees are schizophrenic, the nature of this disorder should be reflected, as hypothesized, in the obtained factor structure.

As has been stated, the general factor accounts for some 40 per cent. of the variance; the remaining factors together account for something less than 20 per cent. of the individual variation.

The highest loading variables on factor two are Group B (Mood Tempo, Neologisms, and Hallucinations) ·56; Delusions ·49; and Associative Processes ·31. This factor appears to be associated with thought disorder.

On factor three, the variables showing the highest loadings are Handshake ·45; Departure ·43; Salutation ·30; and loading negatively Mannerisms —·31. This factor appears to be associated with the patient's ability to establish and

to discontinue contact with the psychiatrist; that is to say, his approach to other people.

Factor four is ill-defined. However, the pattern of loadings suggests that it is a motor factor.

Factor five loads most highly on Thought Blocking .37 and Thought Production .36. This factor is also difficult to identify, but it appears to be associated with an unwillingness to speak.

Factor six shows high loadings on Cleanliness of Nails .76; Cleanliness of Hands and face .67; and Cleanliness of Dress .55 and is clearly a factor of general cleanliness, which may be related to nursing care.

The results of the factorization do not support the hypothesis that the analysis of ratings would give rise to factors presenting patterns of factor loadings such that they might readily be identified as descriptive of the basic clinical sub-divisions of schizophrenia.

The results of this orthogonal analysis yield strong evidence that chronic hospitalized schizophrenia is essentially a unitary phenomenon.

W.A.S. SECOND REVISION

The data presented in this report form the basis of the second revision of the Weyburn Assessment Scale.

The revised scale is intended specifically as an instrument for the assessment of the degrees of illness in chronic hospitalized schizophrenic patients. For this group, it will provide a relatively reliable measure of change of behaviour and will offer an objective assessment of the success or failure of therapeutic procedures.

BIBLIOGRAPHY

- BAKER, A. A., and THORPE, J. G., "Deteriorated Psychotic Patients—Their Treatment and Its Assessment", *J. Ment. Sci.*, 1956, **102**, 780-789.
- CATTELL, R. B., Personal communication, 1956.
- COHEN, L. H., MALMO, R. B., and THALE, T., "Measurement of Chronic Psychotic Over-activity by the Norwich Rating Scale", *J. Gen. Psychol.*, 1944, **30**, 65-74.
- DONNELLY, J., "Clinical Trial of Chronic Schizophrenia Patients Under Nucleotide Therapy", 1953. Unpublished Report.
- EYSENCK, H. J., *The Structure of Human Personality*, 1953. London: Methuen.
- GUERTIN, W. H., "A Factor Analytic Study of Schizophrenic Symptoms", 1951. Thesis, Univ. of Mich., University Microfilms, Ann Arbor.
- Idem*, "A Factor Analysis of Schizophrenics Rated on the Activity Rating Scale", *J. Clin. Psychol.*, 1956, **12**, 163-166.
- LORR, M., "Multi-dimensional Scale for Rating Psychiatric Patients", 1953. Veterans Administration Technical Bulletin, T.B. 10-507, Veterans Administration, Washington.
- LUCY, J., CLANCEY, J., HOFFER, A., OSMOND, H., SMYTHIES, J., and STEFANIUK, W. B., "Design and Planning in Psychiatric Research as Illustrated by the Weyburn Chronic Nucleotide Project", *Bull. Menn. Clinic.*, 1954, **18**, 147.
- MALAMUD, W., and SANDS, S. L., "A Revision of the Psychiatric Rating Scale", *Amer. J. Psychiat.*, 1947, **104**, 231-237.
- MILLER, D., CLANCY, J., and CUMMING, E., "A Method of Evaluating Progress in Patients Suffering from Chronic Schizophrenia", *Psychiat. Quart.*, 1953, **27**, 439-451.
- PLANT, J. S., "Rating Scheme for Conduct", *Amer. J. Psychiat.*, 1922, **1**, 547-572.
- SCHERER, I. W., "A Behaviour Rating Scale for use in Activity Therapy Situations", Info. Bull., Dept. Med. and Surg., Psychiatric and Neurological Div., The Veterans Administration, January, 1951.
- THURSTONE, L. L., *Multiple Factor Analysis*, 1947. University of Chicago Press.
- WILCOX, P. H., "The Gardener Behaviour Chart", *Amer. J. Psychiat.*, 1942, **98**, 874-880.
- WHITTENBORN, J. R., and HOLZBERG, J. D., "The Generality of Psychiatric Syndromes", *J. Consult. Psychol.*, 1951, **15**, 372-380.
- WITTMAN, P. M., "The Elgin Check List of Fundamental Psychotic Behaviour Reactions", *Amer. Psychol.*, 1948, **3**, 280.

TOWARD AN INTEGRATED THEORY OF SCHIZOPHRENIA

By

EMIL G. CONASON, M.D.

New York, N. Y.

and

PERCY E. RYBERG, M.D.

*Clinical Director Falkirk Hospital,
Central Valley, New York*

EVIDENCE is accumulating that schizophrenia is a disease involving "immuno-allergic" sensitization of the tissues of the central nervous system. Variations in its symptomatology may in part depend upon the altered ability of the sensitized nervous tissue to bind or release serotonin, and perhaps other endogenous catechol amines. Accumulation of these products; their rates of turnover; the presence of endogenous enzyme enhancers or inhibitors; specific endocrine dyscrasias; blood-brain barrier conditioners; as well as cellular changes in electrolytes, may all play a part in the process. Genetic defects; lags in maturation; autonomic imbalance and pre-psychotic personality structure may lurk in the background.

That these are not primary is indicated by the fact that the disease process is frequently reversible in response to non-specific immuno-allergic desensitization in much the same way as are somatic allergic conditions.

Conason and Ryberg (1) discuss results of an empirically developed desensitizing regimen in the treatment of 98 mentally disturbed patients, 22 of whom were diagnosed as chronic schizophrenics. This empirical treatment has evolved over the course of some twenty-five years during the treatment of several thousand patients.

Beginning in 1931 with the use of adrenal cortical extract (ACE) in the treatment of a depressed patient suffering with Addison's hypoadrenalism, the procedure was extended to non-Addisonian patients suffering with depression accompanied by marked hypotension. Further extensions involved a wide variety of mentally and emotionally disordered patients. Among these were an increasing number diagnosed by competent psychiatrists as schizophrenics, who had failed to respond to conventional therapies. Assessment of the results obtained with ACE replacement therapy in psychotics was disappointing. Improvement did occur in a fair proportion of patients. It was neither complete nor long-lasting. Relapses were frequent after treatment was discontinued. The rationale for treatment had rested on the notion that these patients suffered from adrenal-cortical refractoriness following long-continued unremitting stress stimulation, with a resulting disturbance in salt and water metabolism. It appeared worth while to see whether the temporary improvement obtained with the ACE replacement therapy might be augmented and prolonged by stimulation of the pituitary-infundibular-adrenal axis. Progressively increasing doses of non-specific-protein was the technique of stimulation selected.

By 1948 clinical results seemed to warrant freezing the procedure to facilitate assessment of the results of treatment. The treatment so fixed consists of

giving 3.0 ml. adrenal cortical extract intramuscularly daily for 12 days followed by weekly intramuscular injections of a non-specific-protein mixture containing 2% solids derived from non-pathogenic micrococci, casein, and purified endotoxin, the mixture having proved more reliable in its clinical effect than any one of numerous single pure antigenic substances tried. Beginning with a sub-reactive dose of 0.2 ml. this antigenic mixture is increased progressively by a weekly increment of 0.2 ml. to doses which in the absence of tolerance are shock-producing (3.6 to 5.0 ml.). Given as outlined the treatment caused no observable side reactions except for occasional warmth and soreness at the site of injection, which was considered a signal to repeat the dose the following week. The response of some psychotic patients to this regimen was impressive. In those patients who responded well to the treatment mental fogging lifted; normal feeling tones and optimism replaced depression; agitation and severe anxiety-tension became less persistent and gradually faded; catatonic posturing and indifference disappeared; disabling phobias and obsessions were submerged and then given up; active hallucinating together with paranoia and delusional thinking were gradually replaced by rationalizations and finally by insight. As the patients improved they began to display interest in the outside world, responded to suggestion, were able to concentrate and make decisions and began to initiate activities of their own. Finally they lost their sense of isolation and depersonalization and with little pressure or urging returned to their normal activities to the point of undertaking responsibilities they had been unable to handle. Only those patients who were able to return to full-time work or school activities were considered to have recovered.

Treatment as outlined above resulted in complete clinical and social recovery in half the psychotic patients. An additional quarter of these patients were symptomatically improved, but failed to recover completely. Despite lessening of many of their difficulties, they did not regain steady normal feeling-tones, could not initiate their own activities, or return to work activities in a consistent manner. The remaining quarter failed to respond or regressed while under treatment. About one in five of those successfully treated relapsed within a year of cessation of treatment. In most of these a second course of treatment resulted again in remission, but in view of their previous relapse they were placed on "maintenance therapy" consisting of a perennial series of "booster shots" given at 21 day intervals, much as are given in the treatment of hay fever on a perennial basis. On several occasions attempts were made to step-up the speed of treatment by either increased increments of the medication or by decreasing the interval between injections. Both resulted in agitation and hyperactivity to such an extent that they were not pursued. On one occasion, three patients who were each making excellent progress were precipitously thrown into severe relapse by the substitution of an oily menstruum for the usual aqueous one, in a single injection. At the time, 1949, no satisfactory explanation for these relapses presented itself. In the light of our present theory it seems possible that the "adjuvant effect" of the oil or its depot action may have caused resensitization of the brain.

Success or failure in treatment could not be accounted for by age, sex, length of illness, severity of initial symptoms, pre-psychotic type of personality structure or clinical diagnosis. Attempts to relate prognosis to evidence of change in pituitary or adrenal output failed to show any consistency. The reaction of patients to the Randolph or Thorn eosinophile test, or their titre of urinary steroids, varied irregularly and gave no reliable clue to the probability of recovery. The only phenomenon which showed up with consistency

was that displayed by patients who had presented definite evidence of salt and water imbalance. These patients consistently signalled oncoming recovery by rapid increase in weight which was interpreted as due to hydration.

Patients with such endocrine dyscrasias as hypo- or hyperthyroidism, Cushing's syndrome or marked hypogonadal states combined with psychosis were treated with appropriate endocrine therapies as well as with non-specific desensitization. These patients were not included in statistics for purposes of appraisal of results of treatment. The lack of a satisfactory rationale of treatment discouraged earlier publication. Recent findings combine to suggest such a rationale. Even though the findings are not conclusive they map out a promising line of study. Thus recent publications show that:

(a) The sedating effects of the active rauwolfia alkaloids are mediated through the release of serotonin from its binding to brain tissue (1b and 3).

(b) Lysergic acid diethylamide-like excitation and hallucinogenic hyperactivity accompanies excess serotonin turnover or accumulation in the brain (3). Such excess may be experimentally induced by feeding the serotonin precursor, 5-hydroxytryptophane (2) or by blocking the serotonin inactivator monoamine oxidase (3).

(c) This has led to the suggestion that disturbances in brain serotonin level are basic to schizophrenia (2). Wooley is testing this theory by feeding 5-hydroxytryptophane to a group of schizophrenics to raise their serotonin brain levels. Unlike serotonin itself, the precursor passes the blood-brain barrier, enters the brain and is there decarboxylated to form serotonin. Free serotonin having discharged its function is instantly inactivated by monoamine oxidase (4). Serotonin bound to tissue escapes inactivation, providing a protected depot for release of the active free form in response to normal needs or to homeostatic feedback demands. The mechanism for the release of serotonin from its binding to normal brain tissue is not known. Its release from somatic tissue-binding (*in vivo* and *in vitro*) in the course of immuno-allergic or anaphylactic reactions suggests that a similar mechanism may induce its release from brain tissue. H. Weissbach *et al.* (5) review some of the evidence of immuno-allergic or anaphylactic release of serotonin from somatic tissue. They call attention to:

(a) The release of serotonin from normal rabbit platelets in plasma on addition of purified antigen or antibody (6).

(b) The relationship between tolerance to serotonin and desensitization in the guinea pig (7).

(c) The release of serotonin in the blood of rabbits during anaphylactic shock (8).

(d) The blocking of serotonin-induced and antigen-induced uterine contractions in the sensitized mouse uterus (Schultz-Dale reaction) by reserpine and lysergic acid diethylamide. The author shows that antigen causes uterine contractions by release of serotonin (9).

The suggestion follows that schizophrenia may involve immuno-allergic or anaphylactic brain sensitization, with secondary disturbances in serotonin brain levels. Such a postulation finds further support in the following:

I. The reported rarity of asthma, hay fever, and acute rheumatic arthritis among populations of mental hospitals as compared to the general population or to non-psychotic hospital personnel (10) together with the sharp alternation

between asthma (and other so-called psychosomatic illness) and psychosis in some psychotics, suggests the likelihood that competition between allergenically sensitized nervous tissue and similarly sensitized somatic tissue for a common incitor is involved.

II. Varying sensitivity of schizophrenics to adrenalin and noradrenalin has led to controversial theories of autonomic imbalance as crucial to schizophrenia (12 and 13). The findings are as susceptible to explanation by immuno-allergic sensitization of the psychotic (with accompanying adrenalin and noradrenalin hyper-reactivity) as they are to theories of autonomic imbalance.

Funkenstein *et al.* (12), for example, classify their patients according to their pressor response to a standard dose of adrenalin together with their ability to resist the hypotensive action of methacholine. They claim to be able to predict success or failure in electroshock therapy by these reactions. Patients with very marked adrenalin sensitivity as shown by excessive pressor response and concomitant good resistance to the hypotensive action of methacholine, doing poorly, while those with lessened sensitivity to adrenalin as shown by a small transient pressor response and profound and prolonged hypotension after methacholine, respond to the electroshock treatment with recovery.

Similar sensitivity to adrenalin can be experimentally induced in rabbits by sensitizing them to bacterial endotoxins (14). Rabbits made sensitive to endotoxins are found to be also sensitive to adrenalin and noradrenalin. In these animals the catechol amines trigger repeated immuno-allergic reactions. Unlike normal untreated animals who react to an intradermal injection of adrenalin by transient skin blanching, sensitized rabbits develop severe haemorrhagic necrosis in response to intradermal adrenalin or noradrenalin. This necrosis is antagonized by such catechol amine inhibitors as chlorpromazine and dibenzylamine, as well as by treatment of the sensitized animal with cortisone for several days. This inhibitory action of cortisone contrasts sharply with the fact that treatment of normal untreated rabbits with cortisone renders them sensitive to bacterial endotoxin so that a single dose of endotoxin following cortisone acts like a challenging dose, producing a generalized Schwartzman reaction as well as sensitization to adrenalin and noradrenalin (14 and 15). Thomas calls attention to the striking parallel between this sensitizing effect of cortisone and the occasional precipitation of psychosis in the course of cortisone therapy implying a similarity in mechanism. The parallel is sharpened in the light of our proposed theory of the pathogenesis of schizophrenia by Thomas's recent suggestion that the generalized Schwartzman phenomenon as well as accompanying adrenalin and noradrenalin reactions are mediated through serotonin release (17).

Thomas was able to induce a "temporary immunologically non-specific state of tolerance" in his sensitized rabbits by repeated daily injections of small doses of bacterial endotoxin. This induced tolerance to adrenalin and noradrenalin as well (14), (15 and 16).

III. Recent findings of organ-specific auto-antibodies in the blood of patients suffering with Hashimoto's thyroiditis, thyrotoxicosis, and Addison's disease (18, 19, 20 and 21) suggest that auto-antibodies to nervous tissue be sought in the blood and C.S.F. of schizophrenics. The induction of schizophrenic-like states in non-psychotics by injection of "taraxein" from the serum of psychotics brings to mind the "passive transfer" of the allergist. The persistence and life-long course of schizophrenia might find explanation in such mechanisms as are encountered in genetically determined iso-immune or auto-

immune states in which iso-antigens or auto-antibodies trigger repeated cycles of immuno-allergic reactions (23).

IV. The tranquillizing effects of an increasing number of anti-allergic drugs of varying chemical structure, such as phenergan, benadryl, chlorpromazine, bonamine, hydroxyzine and others (23 and 24) raises the suspicion that their tranquillizing effects may be related to their anti-allergic action.

V. The reversal of the schizophrenic process by the non-specific desensitizing regimen in 50 per cent. of chronic schizophrenics discussed above adds plausibility to the theory that the disease primarily involves immuno-allergic sensitization of the nervous system, with secondary disturbances in the brain turnover of the catechol amines, more especially noradrenalin and serotonin.

It is suggested that the homeostatic mechanisms which operate to maintain sanity in the normal mind, are blocked in the psychotic by immuno-allergic brain tissue alterations, just as the homeostatic mechanisms regulating somatic tissue metabolism are blocked by such alterations in hay fever, asthma, serum sickness, angioneurotic oedema, contact dermatitis and other so-called atopic allergic states.

REFERENCES

- 1a. CONASON, E. G., and RYBERG, P. E., *Dis. Nerv. Syst.* (Monograph Suppt.), July, 1957, **18**, 3.
- 1b. BRODIE, B. B., SHORE, P. A., and PLETSCHER, A., *Science*, 1956, **123**, 992.
2. WOOLEY, D. W., *Science*, 1957, **125**, 752.
3. SHORE, P. A., and BRODIE, B. B., *Proc. Soc. Biol. Med.*, 1957, **94**, 433.
4. BRODIE, B. B., and SHORE, P. A., *Ann. N.Y. Acad. Sci.*, 1957, **66**, 631.
5. WEISSBACH, H., WAALKES, T. R., and UDENFREIND, S., *Science*, 1957, **125**, 235.
6. HUMPHRIES, J. H., and JACQUES, R., *J. Physiol.*, 1955, **128**, 9.
7. HERXHEIMER, H., *J. Physiol.*, 1955, **128**, 435.
8. WAALKES, T. R., *et al.*, *J. Pharmacol. Exptl. Therap.*, 1957. In Press.
9. FINK, M. A., *Proc. Soc. Exptl. Biol. Med.*, 1956, **92**, 657.
10. EHRENTHEIL, O. F., *Arch. Neurol. Psychiat.*, 1957, **77**, 178.
11. FUNKENSTEIN, D. H., GREENBLATT, M., and SOLOMON, H., *J. Nerv. Ment. Dis.*, 1951, **114**, 1.
12. *Idem*, *Scientific American*, 1955. May.
13. PERRIN, G. M., and ALTSCHULE, M. D., *New Eng. J. Med.*, 1957, **256**, 682.
14. THOMAS, L., *Ann. Rev. Physiol.*, 1954, **16**, 467.
15. *Idem*, *Bull. N.Y. Acad. Med.*, 1955, **31**, 485.
16. *Idem*, *J. Exptl. Med.*, 1956, **104**, 865.
17. *Idem*, *Ciba Medical News*, 1957. 20 May. Press release.
18. ANDERSON, J. R., *et al.*, *Lancet*, 1957, *ii*, 1123.
19. WITEBSKY, E., and ROSE, N. R., *J. Immunol.*, 1956, **76**, 408.
20. ROSE, N. R., and WITEBSKY, E., *ibid.*, p. 417.
21. ROITT, I. M., *et al.*, *Lancet*, 1956, *ii*, 820.
22. LEACH, B., and HEATH, R. G., *Ciba Medical News*, 1957. 3 June; *Amer. J. Psychiatry*, 1957. July.
23. RAFFEL, S., *Immunity*, 1953. New York: Appleton Century Croft.
24. BENDER, L., and NICHTERN, S., *N.Y. State J. Med.*, 1956. 15 September.

MENTAL DISEASE IN AFRICANS: RACIAL DETERMINISM

By

H. J. SIMONS

School of African Studies, University of Cape Town

THERE has grown up in the past 30 years in East Africa a school of medical psychologists which sets out to interpret African thought processes, personality traits and mental disorders in terms of a racially genetic and cultural disposition. A correlation is assumed or suggested between a specific brain structure, social behaviour, psychology and psycho-pathology. Explanations are couched in terms of a racial determinism, and involve an assertion of inferiority as compared with the White race.

Those of us who are acquainted with the corresponding efforts of Wilhelm Wundt and his folk psychologists, or with the disastrous myth of Nordic superiority produced by Gobineau and Houston Chamberlain, will be familiar with the difficulties that must be overcome by the thinker who seeks to establish a causal relationship between race, culture and mental development. These entities cannot be satisfactorily described in empirical terms, there is much doubt as to the bearing of race on individual characters, including body ones, and we have as yet no means of distinguishing inherited from environmental factors in social behaviour.

In so far as the race determinist tries to substantiate his theory by objective observations of a scientific kind, he relies mainly on comparative studies in anatomy and intelligence testing. The East African school, having a strong bias towards medicine rather than psychology, has drawn its evidence almost wholly from anatomical studies, particularly of the African's brain as contrasted with the brain of Europeans.

Indeed, the school's hypotheses may be said to have originated in measurements of African brains. These were said to be smaller, simpler and therefore likely to be inferior in reasoning power and other significant functions as compared with European brains. Much of the theory rested on a conception of the evolution and operation of the different parts of the human brain which is now largely discredited. Le Gros Clark (1952) has pointed out that intellect and emotions are closely interlocked in mental activity and that the allegedly "primitive" regions of the brain play an integral part in this process.

Gordon (1935-36) asserted that the "frontal brain", having evolved last, was normally less stable and durable than other parts of the brain. He claimed to have demonstrated the "frontal inferiority of the Native brain" and adduced this inferiority as the reason for another alleged phenomenon: the "unprecedented stress and strain to the Native frontal brain" caused by scholastic education. This, he implied, was the reason why cases of adolescent psychosis in his series occurred only in educated persons.

An evolutionary approach is also evident in Vint's (1932-33) comparison between the frontal cortex in Africans and Europeans, leading to the conclusion (Sequeira, 1932) that in relation to the corresponding parts of the European's brain, represented by 100, the depth of the East African's "infragranular"

layer is 106, the "granular" 98·7, and the "supragranular" 92. He describes the infragranular layer as the seat or physical basis of the "animal instincts", the granular layer as that of "perception", and the supragranular as being concerned with the "higher" functions of the will and intellect.

"In the East African therefore", claimed Sequeira (1932) commenting on these findings of Vint, "animal instincts are provided with six per cent. more physical basis than in the European, but the physical basis of 'mind' shows a preponderance in favour of the European of 9·3 per cent." There follows a plea for racial discrimination, which underlies and indeed seems to be the main purpose of much writing of this kind.

"If it is proved that the physical basis of 'mind' in the East African differs from that of the European, it seems quite possible that efforts to educate these backward races on European lines will prove ineffective and possibly disastrous. It has long been recognized among highly civilized races that the educational methods applied to the normal child cannot be applied to the backward or defective."

I am not in a position to assess the value of these anatomical observations. It would seem, however, that the mere assertion of a structural difference as here outlined would not demonstrate a qualitative difference of intellect or psychical traits. The somatic types compared may have different standards of physical normality.

This objection also applies to the comparison made by Vint between the brain of an adult African and that of a European boy aged 7 or 8 years. The latter, he says, is 84 per cent. of the fully developed adult European brain. The "supragranular cortex" of the adult African is similarly 84 per cent. of the European's. Therefore, concludes Vint, "the stage of cerebral development reached by the average Native is that of the average European boy of between 7 and 8 years". This statement is, of course, incorrect and misleading. The one brain is that of a child, the other that of a man; and the two differ in terms of the whole range of qualities that distinguish the child's immature and unrealized potential from the adult personality.

Dart (1956) has recently drawn attention to the fallacy of comparing brain sizes unrelated to body volume, an aspect that the East Africans appear to have overlooked. His masterly survey suggests that the conclusions drawn by the East Africans as to the relation between brain size and intelligence are untenable in the light of present knowledge.

Later adherents of this school, though more discreet than its founders, still rely on their findings to support an assumption of African inferiority. Carothers (1953) cites Vint to the effect that the African's brain is about the same as a European boy's, and regrets that "this fine piece of pioneer research did not inspire something more than criticism". Smartt (1956), even more restrained, suggests only "the possibility of actual structural differences in the brains of Africans—particularly in the diencephalic frontal system—which may produce psychopathic tendencies. If these differences exist we have no way of making them good."

CULTURAL TRAITS

Unable to fire enthusiasm in scientific circles for its anatomical speculations, the school has shifted its emphasis from the plane of physical anthropology to that of social anthropology. Here the race determinist treads on even more dangerous ground, and usually does so with less training and experience than he possesses for his hazardous ventures in comparative human physiology. The

study of tribal society has become a highly specialized field of research and follows approved scientific methods. Persons not equipped for the exacting field work required and without the necessary theoretical insight are hardly in a position either to make more than haphazard, isolated observations or to add significantly to the interpretations of social anthropologists.

Consistent with their notion of an "African brain", members of the school assume the existence of an "African culture". Carothers, in a monograph (1953) prepared for the World Health Organization, claims to have extracted "the fundamental and distinctive features of African culture" south of the Sahara.

Carothers does not give a definition of "fundamental", but seems to exclude law, government, economic activity, and kinship. His own comments on the results of his efforts suggest that the phrase "fundamental and distinctive" conveys to his mind the idea of something exotic—"odd" as he puts it—in comparison with the standards of an urban, industrialized community in Western Europe.

"The subsequent description may give the impression that African culture is entirely odd, alien to anything one knows in the Western world. Such an impression would be false, for much of African life is just like life elsewhere. But in a monograph of this nature, it is relevant to describe only peculiarities and it must in general be assumed that life is otherwise much like that in rural Europe. Indeed, some of the peculiarities themselves would not be regarded as alien by certain elements in Europe" (p. 42).*

Data selected according to these criteria are bound to yield a distorted and misleading account of African societies. The margin of error has been further widened by Carothers's tendency to generalize from inadequate or unrepresentative data. His descriptions of alleged "African" customs are not sufficiently documented and cannot be accepted without more ado as being true of all or most African societies. Indeed, it is doubtful if the general run of his statements, of which a few typical examples are quoted below, can be regarded as true without much explanation and qualification of even particular societies.

"Weaning is abrupt, the more so in that there was previously no constraint in feeding" (p. 97).

The child "meets no restraint, his wants are met at all times, and his developing functions are encouraged at all times" (p. 52).

"Whereas, prior to initiation, masturbation was considered right and proper (at least for boys), thereafter it is regarded as childish and intercourse between the sexes is correct" (p. 47).

"Illegitimacy has little meaning for the child in Africa" (p. 48).

"Certainly there is little link in Africa between ethics and religion, and morality is simply a question of the application or contravention of the traditionally correct rules of social behaviour" (p. 51).

Some of these statements are patently improbable, ambiguous and inconsistent, but only by consulting the relevant literature could one decide how far they are true of, for instance, the Islamic Hausa, the pigmy Batwa, the aristocratic Watuzi, the supercilious Masai, the highly organized Baganda, or the thrifty Xhosa. Special mention should however be made of two persistent errors, long ago exposed by social anthropologists, but still recurring in popular, unscientific writing. According to the one error, people at the tribal stage are illogical or "pre-logical"; according to the other, they are identical robots.

In "African culture", writes Carothers, "logic is distorted, cause and effect are confused, and the question 'Why?' must always be answered in magical and animistic terms which do not permit of further speculation . . . Logic, speculation, and the search for true causes are supplanted by magic and animism,

* All page references shown in the text relate to Carothers (1953).

which supply the answers and the lines of action" (p. 53). Smartt (1956) adds: "the average rural native of Tanganyika . . . is filled with superstitious beliefs and his thinking is animistic, concrete, and often illogical."

So it probably is, like the thinking of all people on occasion. But one must protest against the claim that metaphysical thought is necessarily illogical. It is essentially pre-scientific—perhaps embodies truths about the unconscious that elude the attention of European materialist science—but magical beliefs are inherently consistent. They are deduced as logically from the underlying premises as are the dogmas of any other religious system. Indeed, a case could be made out for tribal thought as being the more unified and consistent, since it is not conscious of the dichotomy between supernatural faith and scientific empiricism that plagues Western man. Nor is it true to say that magic rules out speculation or even experimentation. The diviner and herbalist try both, looking for new techniques and remedies or applying old ones to new situations. Their adaptability is one important reason for their survival in a crumbling tribalism.

The claim that Africans conform habitually and spontaneously to rules is linked to an assertion of physical sameness. Carothers (1951) and Smartt (1956) believe that a rigid social organization is universal in Africa, demands meticulous obedience, and makes every member of the group think and act like the rest. No African is ever in a position when he does not know how to behave or what to do. This being so, writes Smartt, the personality of one African should be much like the rest; even "his physique shows little variation. There are differences in stature between various tribes but the types of physique described by Kretschmer are not often found in Africa."

This uniformity is traced back to the dawn of the Mesolithic age in East Africa! "During an estimated 10,000 years the rural African has remained almost unchanged."

10,000 years ago on the archaeological scale was the time of the Upper Pleistocene geological stage, the end of the Gamblian pluvial climatic stage, and the start of the Middle Stone cultural stage. It was the divide between Paleolithic and Neolithic man, or, in Gordon Childe's terminology, between Savagery and Barbarism. It was within this span that *Homo Sapiens* accomplished his greatest and most revolutionary advance: the transition from hunting and food collecting to the growing of crops, the rearing of domesticated animals, and the formation of relatively big, stable communities. The Neolithic revolution is thought to have begun in the Fertile Crescent of the Middle East, between 6000 and 5000 B.C. It is certainly after this time that the great changes associated with the introduction of food production and stock breeding spread southward into East Africa.

We must reject with equal emphasis the notion of a changeless East African man during the historical period. Starting from about 700 B.C. when Arabs are known to have traded along the coast, we have to take note of the occupation of large parts of East Africa by advanced peoples of Hamitic origin at the beginning of this era, the spread of Christianity into Abyssinia about 1,600 years ago, the coming of iron before about 1,000 years ago, the ravages of the slave trade in the modern period, and the coming of the White man. Our knowledge of East Africa's past is negligible, but it is enough to establish the presence and interaction of peoples of different physical and cultural types during the past 2,000 years.

We must conclude that traces of these divergent strains still occur in East African populations and that the assumption of complete uniformity is un-

founded. Should the absence of Kretschmer's types be established, an empirical classification of African physiques may show the existence of other somatic categories.

A SYNTHETIC PSYCHOLOGY

If we agree that the many ethnic groups south of the Sahara do not have the same practices, beliefs, ideas and sentiments, we must necessarily reject the notion that they share a common psychology.

This corollary has evidently been grasped by Diedrich Westermann, whose book *The African Today and Tomorrow* is one of Carothers's main sources on the nature of African psychical traits. In the first English edition, published in 1934, the chapter on "The Negro's Mind" begins with the observation:

"If the Negroes are one race, it is permissible to inquire into the characteristic traits of this race."

The sentence was changed in the second edition of 1939 to read:

"Although the peoples whom we call Negroes do not form one race in the strict sense of this term, and in spite of the fact that they have evolved widely differing cultures, they share so many essential physical and cultural features that (with the necessary reservations) they can be called a unit, into the characteristic traits of which it is permissible to inquire."

The whole chapter has been omitted from the latest edition, published in 1949. Carothers took his extracts from the second edition.

Most physical anthropologists regard the Negro as being among the most varied racial types. Even if Africans did form a single race—a proposition to which Westermann never subscribed—we should be chary of believing that they have a social or collective consciousness, or a "racial" psychology. There is no evidence to support an assumption that the evolutionary process presumed to have produced the African's peculiar bodily characteristics also produced a set of racial psychical characteristics. Would not selection and heredity have favoured a diversity of mental qualities, or at any rate the higher grades of intelligence? It is surely not in dispute that Africans differ in intelligence, temperament and personality. Anyone wishing to discover an African psychology would have to find a satisfactory method of observing and recording these traits and their distribution in the population at large.

Carothers, Smartt, and other members of the school do not claim to have used a measuring-rod of psychic quantities. They rely on general, unverified impressions and popular conceptions current among colonists and officials, and similar in kind to the 17th century encyclopaedist's description of the Cape of Good Hope's inhabitants: "They have Sense in their Looks, but none in their Brains." Carothers says that these impressions "represent the truth", but they could be more accurately described as a social stereotype.

The social stereotype is a conventional image current in a group and generally accepted by its members as a fair portrayal of the essential qualities of persons belonging to another group. Originating in the idle tattle and common chat of gossips, it represents a crude and popular form of sociological inquiry and discourse. To borrow a phrase of Jung's, it is a "calculus of subjective prejudices", and tells one more about the attitude of the person applying the stereotype than about the person to whom it is applied. In a colonial-type society, stereotypes are used to support the assumption of intrinsic superiority in the White group, and so justify racial discrimination and privilege.

Stereotypes are products of ignorance as well as of prejudice. A person is less likely to make facile generalizations about members of his family, firm or class, whom he knows well and whose distinctive traits he recognizes. It is when the observer stands outside the group, feels little sympathy with it, and sees its individual members in the mass, that he ventures to apply to them collectively the terminology evolved for the analysis of individual personalities.

The reader will judge for himself whether the following passage constitutes a stereotype. It is Carothers's notion of "African mentality" as seen by "most" White observers.

"The African accordingly has been described as conventional; highly dependent on physical and emotional stimulation, lacking in spontaneity, foresight, tenacity, judgment and humility; inapt for sound abstraction and for logic; given to phantasy and fabrication; and, in general, as unstable, impulsive, unreliable, irresponsible, and living in the present without reflection or ambition, or regard for the rights of people outside his own circle" (p. 87).

Such phrases do no doubt circulate among settlers and officials, but cannot for that reason alone be regarded as "representing the truth". Indeed, they are not convincing. Are we not all "highly dependent on physical and emotional stimulation"? "Conventional" people may be "given to phantasy and fabrication" in an inner, concealed life, but they are not usually "unreliable and irresponsible". Nor is an "absence of spontaneity" associated with "instability and impulsiveness". People who lack "humility" are presumably self-assured, perhaps aggressive. Are not these qualities often attached to tenacity and ambition? Does an African "live in the present without reflection or ambition" when he sets out to acquire cattle, obtain a wife, buy a bicycle, or send his children to school?

MENTAL DISORDERS

The speculations of the East African medical school are used to provide a theory of African mental disorders. These are described in conventional Kraepelinian terms; their incidence is assessed largely by an examination of admissions to mental hospitals; and they are compared with mental disorders in Europeans and at times in American Negroes. The differences are attributed to the alleged cultural, anatomical, or psychological peculiarities in Africans.

The survey of known mental cases leads to the conclusion that the incidence of "insanity" in Africans is very low: "probably very much lower than that in Europe or America" (Carothers, 1953). This applies to nearly all kinds of insanity. As regards the organic types, both general paralysis and arteriosclerotic dementia are rarely recorded. Paranoia, paraphrenia and depressive psychoses are seldom seen. Obsessional neurosis is almost unknown. Overt anxiety symptoms are not prominent in psychoses and are almost absent in depressive states. Schizophrenia is the most common psychiatric disorder, but its recorded incidence is far lower than in Europeans.

This is the picture drawn by the East African medical school on admittedly scanty information. As will be shown later, there is reason to doubt whether the picture is altogether a true reflection of reality. But in the form presented, it reveals a people far freer than Europeans from most if not all kinds of mental ailment. The difference may well be significant and require an explanation.

Psychiatrists are not able to account for the difference. Conjecture has to take the place of established theory, and there is room for surmise and supposition. One might, for example, draw the inference that unstable types have been

discouraged by some untutored process of eugenics, and that Africans are more balanced, healthier in mind, than the European stock, and therefore better equipped for great social change and stress.

Such hypothesis would discredit the conventional stereotype of African inferiority. The East African school has preferred other explanations. One, which may be called the sociological, is consistent with its notions of tribal society, and is expressed by Carothers (1947) as follows:

"the very low general incidence of insanity among Africans living in their natural environment is due to an absence of problems in the social, sexual and economic spheres."

The proposition includes two assertions of fact and an assumed association. The first assertion has not been satisfactorily demonstrated; the second is incorrect; and the alleged association is inconsistent with psychiatric theory concerning the unconscious and its relation to psychosis.

In general, it should be pointed out that we know of no people who are free of the cares associated with death, birth, nutrition, reproduction and security. Further, that the low incidence of mental diseases is supposed to occur in present-day Africa, which is not free of other problems. Carothers's statement appeared in print in 1947. At this time thousands of Africans from Kenya and Tanganyika, who had been enlisted, trained and employed to fight a ruthless war with modern weapons, were undergoing the difficult, painful process of adjustment to civilian life. The society to which they returned was in a state of disorganization, and stood poised on the verge of a long, brutal civil war, explicable only in terms of acute, critical social conflict.

Carothers has laid himself open to the reproach of having created an imaginary society, less ingenious than but as unreal as the worlds that his patients create in their phantasies. They wish to escape from reality; his aim is to support a preconceived notion of African inferiority.

His second line of approach proceeds from certain psychological and anatomical speculations and leads to the startling conclusion that "*all* primitive Africans are psychopathic by European standards" (1951). Here "primitive" presumably means tribal, but they are not the only ones held to be abnormal in their normality, for, says Carothers, the mentality of "the partially detribalized African"—that is the great majority—resembles to an even greater degree "a certain type of aberrant European mentality commonly included under the title psychopathic".

The psychiatrist will decide if "psychopathic" is a specific condition capable of precise definition. The sociologist must however enter a protest against the stratagem of assessing "abnormal" conduct in one society by the norms current in another, very different society. It is common observation that foreigners usually seem eccentric to the vulgar mind. There is much in European behaviour patterns—their strongly developed acquisitive drives, excessive stimulation of eroticism, and aggressiveness in inter-group relationships—that many Africans find inexplicable and "abnormal". But who would accept the African's judgment as a correct diagnosis of European mental health?

The anatomical reference in the Carothers theory (1951) can be summarized in the following propositions: the main function of the frontal lobes is to integrate stimuli from the thalamus and cortex; Africans make little use of their frontal lobes except for verbal synthesis; this idleness is the cause of "all the observed African peculiarities" and the "complete" resemblance between leucotomized Europeans and "primitive Africans". The first two of these

assertions raise issues that concern the anatomist, physiologist and psychiatrist; the third proposition falls within the domain of the sociologist.

The objections raised to broad, impressionistic, unsubstantiated accounts of African personality traits apply also to the comparison between leucotomized Europeans and normal Africans. The whole approach seems ill-conceived and unfortunate. It is not improved by the failure to give chapter and verse. Indeed, we are given even less to go on than in the assessment of African psychology. Carothers (1951) explains:

"It would have been possible to write the analysis of the leucotomized European's mentality in words that echoed the analysis of the African's; but it would have been wearisome reading."

Some of us would willingly pay in tedium for precision and proof. The feat suggested might be possible, but would the words reflect reality? The samples given are not encouraging. The opinions of (three) unrepresentative employers are not the best source of information on the reliability of their servants. An alleged irresistible impulse in Africans to laugh uproariously at blind men slipping on bananas, or to jolly a condemned murderer about his fate, is surely insufficient evidence of a deficiency in "social sense". The fondness shown by a single leucotomized European for the company of Africans can be attributed to reasons other than a mental affinity between them.

There is real danger in such writing. It is liable to pass into technical literature, and become an "authority" for subsequent writers. Carothers quotes Carothers, and is quoted in turn by Smartt (1956) as in the following passage:

"It is difficult to deny the similarity of the bush African's mentality to frontal lobe defect syndrome, or the 'frontal lobe idleness' postulated by Carothers. Many Africans tend to be apathetic and lack interest in everything except their immediate environment. Initiative is usually blunted and may be limited to finding enough to eat and a shady tree under which to rest. In his work with Europeans he is often found to be irresponsible and unreliable. Under the least mental or physical stress he shows a tendency to uninhibited conduct."

It should not be necessary to point out the dangers of sweeping and immoderate statements on important issues. "Many" may mean most, half, or perhaps less than half; "tendencies" are inclinations or trends, and these may be restrained, obstructed by contra-tendencies; the African's need of nutrition and sleep is satisfied by a wide range of activities requiring the display of energy and effort; the smallest degree of stress may be insignificant, and persons who are uninhibited to this extent are likely to be regarded in law as insane.

THE INCIDENCE OF MENTAL DISORDERS

It is doubtful if we know enough about African mental illness to make useful comparisons with the European pattern. Medical scientists generally have some difficulty in defining mental disorders, and their notions are not the same as the African's. Methods of treatment also differ in the two societies. Facilities for modern types of treatment are not available for a great many Africans who, by approved European standards, need psychiatric therapy. Indeed, Africans are often reluctant to seek and accept such care. Consequently, the incidence, distribution and pattern of mental diseases in the African is imperfectly understood.

The approach to the illness known in South Africa as *thwasa* illustrates the difference in conception and treatment. The verb *ukuthwasa* means in Zulu and Xhosa to come out, renew—as said of the new moon, or start of a new season

—and also to initiate a person into the mysteries of divination, from which process he is said to emerge as a new being. *Thwasa* also describes a state, a condition marked by pains in the limbs and “all over” the body, nausea, loss of appetite, hallucinations, wild talk, nervousness and excitability. Dreams figure largely in the symptoms of *thwasa*.

The indicated course of treatment is a period of training, perhaps for a year or more, under the guidance of a leading diviner. This culminates in an initiation ceremony, after the novice has experienced a dream or series of dreams, which, as described, follows a set pattern involving such objects as cattle, snakes, birds, stones, spears.* These are symbolically interpreted as communications from the ancestral spirits, believed to urge and guide the afflicted person through the initiation. Africans say that if a person so “called” fails to undergo the training prescribed, he will surely be punished by loss of reason.

Laubscher (1937), an experienced psychiatrist with special knowledge of certain aspects of tribal society, considers that the condition of a *thwasa* patient just prior to being “called” resembles closely a catatonic or depressive phase in schizophrenic or manic-depressive or epileptic psychosis.

It will be seen from this short account that a particular psychological condition can be viewed in very different ways. What a European considers to be a symptom of acute psychosis is to the African a certain sign of rare mediumistic or other undefined psychic qualities. The European certifies the afflicted person as “insane”; the African regards him as a suitable candidate for a highly valued and honourable profession.

For these and other reasons Africans are often reluctant to undergo treatment in mental hospitals. This statement is confirmed by Laubscher, whose observations carry weight by reason of his psychiatric experience and field-work. This started from a point of view different from the one adopted by the East African school. He, like Tooth (1950, p. 26) thought that an investigator into mental abnormalities should have knowledge of the normal.

“I realized at the very outset that it would be futile to enquire for evidence of mental disorder by employing our conceptions of mental disease, so the first study was devoted to ascertaining the native’s conception of mental disorder, and as a result of these findings a method of approach was developed, based entirely on his conceptions. The enquiry hence entered a sphere of his thinking and concerned itself with his beliefs without disputing the reality of his beliefs.”

The inquiry showed that mental disorders are not rare among tribal Africans, but are assumed to be so by people who “lack intimate contact and comprehension of the culture of these peoples”. The European’s ignorance of African beliefs, language and behaviour, is intensified by the secrecy surrounding the treatment of *thwasa* patients, who are carefully guarded in isolation, perhaps for years, while under the care of the diviner. The administration usually discovers the existence of the patient and takes steps to have him certified only when he commits a crime, or becomes a nuisance to traders, farmers and other Europeans in the neighbourhood, or becomes so violent and uncontrollable that his own relatives and friends ask officials to intervene.

We should not jump to the conclusion that East Africans also try to keep their mentally sick away from the attention of the White administration. Indeed, Carothers (1947, 1953), who used government chiefs to make a census of insane persons, suggests that the possibility of obtaining tax exemptions on ground of disability probably worked in the opposite direction. It is not however

* For an account of *thwasa* dreams and initiation see Kohler (1941) and Tooke (1955).

clear from his account what benefit the chiefs would derive from exemptions granted to individual tribesmen, or what criteria they used in diagnosing insanity. He concedes that they might have missed some simple cases of disorder, but thinks that the objection to a layman's diagnosis is less valid in mental than in most physical disorders. The South African material clearly indicates the contrary view.

It seems that no satisfactory survey has yet been made of mental disorders in any African community. The bulk of the statistical evidence comes from the records of mental hospitals. To assess their value, we could hardly do better than to consider the position in South Africa, which has probably the best medical and hospital services on the continent.

The annual reports of the Commissioner of Mental Hygiene show a tendency over the past 15 years for the relative number of registered European mental patients to decline and the number of non-European patients to increase.

ADMISSIONS AND REGISTERED PATIENTS ADMITTED TO MENTAL HOSPITALS PER
100,000 OF POPULATION

		<i>Direct Admissions</i>		<i>Registered Patients on 31 December</i>	
		1940	1955	1940	1955
Non-European	..	21.3	20.4	101.9	111.4
European		50.1	53.1	238.6	188.0

The table suggests that Europeans are more prone to mental disease, and secondly, that their greater susceptibility is diminishing. The statistics are not however a reliable guide, for there is a chronic and critical scarcity of accommodation, especially for African and Coloured mental patients. "The admission rate", stated the Commissioner (1947), "is entirely controlled by the discharge rate." The number of patients in hospital is likewise determined by the amount of accommodation available and the extent to which the administration allows overcrowding. The rated capacity of mental hospitals was 14,122 at the end of 1954; the excess of patients over maximum rated capacity was 3,060 (Cluver, 1951); and the estimated additional number of "insane" persons requiring accommodation was between 3,000 and 4,000 (1956 *Senate Debates*).

The accommodation is so inadequate that 1,628 non-European and 144 European patients were kept in police cells and gaols until a place could be found for them in hospital. In most instances they were locked up because they were regarded as a public danger or nuisance.

This criterion also largely regulates the admission of non-Europeans into mental hospitals. Indeed, it is probably true to say that throughout the continent African mental patients are being admitted to institutions only when violent, uncontrollable, criminal or destitute (Shelley and Watson, 1936; Tooth, 1950). The "protection of society", not the treatment of the sick, is the main concern. The same principle applies to non-European mental defectives, senile and arteriosclerotic cases. There are no special State institutions for them; they are certified only if very troublesome, and admitted to a mental hospital only if vacancies occur.

The non-European mental hospital population is therefore a highly selected group. The African section particularly rarely includes cases of simple senile dementia, uncomplicated mental defect, alcoholic dementia, paranoia, or schizophrenia simplex (Lamont and Blignaut, 1953). In comparison with

European mental patients, for whom accommodation is usually available, the proportion of African schizophrenics is high (Moffson, 1954). This has led some observers to assume that there is a significant racial difference in the pattern of mental disorders. It seems however that the difference is due not to an African or European idiosyncrasy but to social and administrative policies.

There are other consequences of inadequate treatment that bear upon our problem. Patients who would recover quickly if treated early might, when left untreated for a long time, develop an incurable illness. They then become certifiable and eligible for admission to hospital. A bad organization for the rehabilitation of remitted psychotics may similarly result in a poor prognosis. Such defects are far more common in the treatment of non-Europeans and therefore contribute to the observed difference between them and European mental patients.

Administrative arrangements also partly account for the predominance of African mental patients from towns and other areas in close proximity to European communities. It is in these regions that the African disordered person is likely to be brought to the attention of the administration as a "troublesome person". Little initiative is shown in discovering sick persons in tribal areas who are a burden and nuisance only to relatives and neighbours.

One must assume that, as the absorption of Africans in a modern type of social organization proceeds, and as the standard of health and administrative services improves, the number of mentally sick Africans discovered and treated will increase, while the difference between observed African and European patterns of disorder will diminish.

Both early and contemporary observers have found reason to doubt whether there is an increase in the actual incidence of mental illness among educated, civilized Africans as compared with the tribesmen (Greenlees, 1905; Tooth, 1950). It is too early, however, to draw hard and fast conclusions. The hazards and frustrations of civilization in the form encountered by Africans are not conducive to the growth of harmoniously balanced personalities.

The relative weight of the contributory factors cannot be assessed by examining hospital patients alone. Field work is needed in different kinds of African communities—tribes, urban "locations", farm workers, mining compounds—to discover the incidence of mental illness and social attitudes towards sick persons. The employment of African psychiatrists for this purpose would be invaluable, if not indispensable. They would combine, as few other practitioners can do, the required knowledge of medicine and psychiatry with an intimate knowledge of the people's physiognomy, language, and traits.

SUMMARY

Theories of certain East African medical writers, concerning mental illness in Africans are reviewed. Doubts are raised as to the soundness of certain speculations about African anatomical, cultural, and psychological traits and their relation to mental disorders. Reasons are given for supposing that the incidence and pattern of these disorders are largely unknown or imperfectly understood. Systematic field work by African psychiatrists is suggested.

REFERENCES

- African Studies*, 1955, **14**, 16–22.
 Ann. Rep. Commissioner of Mental Hygiene, 1947, U.G. 22'49, 4.
Ibid., 1954, U.G. 9'56, 2.
 CAROTHERS, J. C., *J. Ment. Sci.*, 1947, **93**, 548–597.
Idem., *ibid.*, 1951, **97**, 12–47.
Idem., W.H.O., 1953, Monograph No. 17.
 CLUVER, E. H., *Social Medicine*, 1951, 454.
 DART, R. A., *S.A. J. Med. Sci.*, 1956, **21**, 23–45.
 GORDON, H. L., *E.A. Med. J.*, 1935/36, **12**, 327–35.

- GREENLEES, T. D., *S.A. Med. Rec.*, 1905, **3**, 217-24.
KOHLE, M., *The Izangoma Diviners*, 1941. Union S.A.: Eth. Pubs.
LAMONT, A. M., and BLIGNAUT, W. J., *S.A.M.J.*, 1953, **27**, 637-39.
LAUBSCHER, B. J. F., *Sex, Custom and Psychopathology*, 1937, **5**, 222.
LE GROS CLARK, W. E., *Proc. Conf. Prospects in Psychiatric Research*, 1952. (Ed.: J. M. Tanner.)
MOFFSON, A., *S.A.M.J.*, 1954, **28**, 662-666.
Senate Debates, 1 March, 1956. Col. 1275.
SEQUEIRA, J. H., *Brit. Med. J.*, 1932, *i*, 581.
SHELLEY, H. M., and WATSON, W. H., *J. Ment. Sci.*, 1936, **82**, 701-30.
SMARTT, C. G. F., *ibid.*, 1956, **102**, 441-466.
TOOKE, W. D. HAMMOND, *Afr. Stud.*, 1955, **14**, 16-22.
TOOTH, G., *Studies in Mental Illness in the Gold Coast*, 1950. H.M.S.O.
VINT, F. W., *E.A. Med. J.*, 1932/33, **9**, 30-49.

POST-OPERATIVE PSYCHOSES

By

E. STENGEL, M.D., M.R.C.P.

*Professor of Psychiatry
University of Sheffield*

B. B. ZEITLYN, M.D., D.P.M.

*Consultant Psychiatrist
Addenbrooke's Hospital, Cambridge*

and

E. H. RAYNER, B.Sc., Ph.D.

*Psychologist
The Cassel Hospital*

*From St. Francis' Hospital Observation Ward, The Bethlem Royal Hospital
and The Maudsley Hospital, London**

THE term "post-operative psychosis" refers to mental illnesses which follow in the wake of surgical operations. These conditions have received relatively little attention from psychiatrists. The text-books either do not mention them or, if they do so, consider them with the delirious or toxic-exhaustive states.

Although Ambroise Paré in the 16th century is credited with the recognition of post-operative psychosis, Dupuytren's in 1833 was the first comprehensive account. He wrote "... and finally the brain itself may be overcome by pain, terror or even joy, and reason leaves the patient at the instant when it is most necessary to his welfare that he should remain calm and undisturbed".

Constitutional predisposition and infection were early invoked to explain the post-operative psychoses, and with the advent of anaesthetics and antiseptics these too were added to the list. Psychoses following chloroform and ether anaesthesia, and associated with iodoform and carbolic acid antiseptics came to be reported quite frequently (Savage, 1887; Collins, 1888). The inclusion of entities such as delirium tremens did nothing to clarify the position, which became further complicated by the fact that cases of obvious toxic, infectious or hormonal origin were classified under this heading by some, but excluded by others. In contrast to this approach, the contributions of the surgeons such as Dent 1899, Kelly 1909 and Da Costa 1910, require emphasis. They stressed the role of the dread of operation, and the worries and anxieties associated with it, which was later to receive so much attention.

Kleist (1916) drew attention to the occurrence in many cases of an interval between the operation and the onset of the psychoses. He named this group the "*Interval Psychosis*", while Doyle later distinguished it from the post-anaesthetic and organic psychoses which are alleged to follow immediately upon the operation. Today the multiplicity of responsible factors is generally accepted (Washburne and Carns, 1935; Abeles, 1938). Biochemical, metabolic and vascular changes no less than the various psychological implications of surgery have been recognized as of importance in the aetiology of the post-operative psychoses.

Anaesthetics have for long been blamed for the post-operative psychoses, which can however occur when no anaesthetic at all is used. Dent (1899) dealt with this question—and anticipated Doyle and Kleist in some respects—when he wrote: "There is no doubt that anaesthesia may produce insanity, but I cannot believe that in cases where complete mental recovery takes place after the anaesthetic, and where the mania occurs after a distinct lucid interval of time, the anaesthetic has anything to do with the matter."

The post-anaesthetic syndrome, reviewed by Alfred Meyer (1944), is further distinguished by its clinical picture, for together with such symptoms as confusion, coma, and even progressive dementia, there occur as well signs of involvement of the central nervous system.

* This investigation was carried out when two of the authors (E.S. and B.B.Z.) were members of the staff of these hospitals.

These vary according to where the lesions of selective anoxia are sited (Batten and Courville, 1940) and they have included motor, basal ganglia, and autonomic disturbances (Steezman, 1939).

The site of the operation has been regarded as important with regard to the incidence of subsequent mental disorders. Eye operations are of especial interest, not only because post-operative psychoses have been supposed to occur more frequently than after operations elsewhere, but because of the obvious importance here of psychogenic and environmental factors. The dangers of post-operative blindfolding, with the consequent darkness enhancing the frightening aspects of the operation, quickly became realized. Besides blindfolding, which alone without operation may yet produce psychological symptoms, the age of the patient, usually over sixty, is significant. The rigidity of the personality then, limits the adjustment possible to the experience of operation. Bandaging the eyes may now produce a profound loss of security and orientation and even panic and "experimental" confusion. Characteristic of this condition is a fearful suspicious misinterpretation of the environment (Parker, 1913; Greenwood, 1928). It has been pointed out, however (Preu and Guida, 1937) that not all psychoses after eye operations are necessarily due to psychogenic panic. Should no improvement occur within two days of the removal of the bandages, some other psychosis—arteriosclerotic, or even schizophrenic or affective must be considered.

The particular liability of *gynaecological operations* to produce psychological sequelae remains widely accepted today, despite assurances that, endocrine effects apart, this be not the case (Sullivan, Zaki Ali, 1941). The relation of genital to mental disorder is noteworthy for the many fluctuations and changes of opinion that mark its history, so that what was by some regarded as a cure later was thought to be the cause of mental disease (Barnes, 1916). Keith (1889) condemned the removal of the uterus and ovaries because he claimed that insanity followed in 10 per cent. of those operated upon. Lawson Tait (1889), of the many who disagreed with him, pointed out that the incidence of insanity was no larger than that in the female population generally. The facts according to Hack Tuke's *Dictionary of Psychological Medicine* (1892) strongly supported the proposition that it was the deprivation of the uterus and ovaries, and not the mere surgical operation that led to insanity: "to unsex a woman is surely to maim or effect injuriously the integrity of her nervous system." In contrast there was the belief that pelvic diseases cause mental illness (Rohé, Hobbs, 1897; Taussig, 1912); so that gynaecological surgery became for a while a rational method of psychiatric treatment. Critics however were quick to point out that where relief obtained, success was more likely due to the special nursing attention received than to the operation itself (Russell, 1897). It is generally held that a disproportionate amount of emotion is aroused in any procedure concerning the genital organs, and that these operations therefore result in psychological complications more often than do others. The higher incidence of psychosis after lower bowel and genital operations has also been remarked upon, while Lindemann (1941) reported a special depressive reaction in women occurring twice as often after lower abdominal operations than after those on the upper abdomen.

The question has been raised whether the *age* of the patient, rather than the site of the operation was perhaps the risk-carrying factor (A. Lewis, 1955) and this must be taken into account, with other factors, in determining the importance or otherwise of gynaecological surgery.

Psychological factors. The contributions of Dent, Da Costa and Kelly have already been noticed in this respect. "A history of prolonged worry and dread of operation is the most influential cause of post-operative mental disturbance" (Kelly). "Many patients were worried and depressed and shrank from operation. When such a subject of anxiety comes into hospital he or she is unreasonable, irritable, suspicious and exacting . . . in one so predisposed a shock, loss of blood or any toxic influence or sepsis may be the last touch which finally overthrows the mental balance" (Da Costa). Dent too discussed the fear of operation and he ascribed the effects of gynaecological surgery to the contemplated rather than to the actual loss of function that might ensue.

More recently the organization of the personality as well as pre-operative anxiety and worry have been considered to be important (Abeles); abnormal psychological reactions being regarded as due to the largely unconscious significance of the operation for the individual rather than to the overt stresses he experiences. It has also been pointed out that the patient's unconscious needs influence the decision to undertake surgery as well as the outcome of it (Menninger, 1934). Helene Deutsch showed how great the disparity can be between the "real" danger of the operation and the "inner" dangers it may represent for the patient, and in consequence apparently minor surgery may prove more disastrous than a major procedure with its many more obvious dangers.

The role of fears of mutilation and death, castration wishes and anxieties, and the significance of the diseased organ both in reality and symbolically have all been described, but the claim that pelvic surgery, presumably on the basis of its specific emotional implications, has a particular liability to cause psychoses, requires further assessment.

Symptomatology. Acute and subacute organic reaction-types are most commonly held to constitute a post-operative psychosis. Although the non-specificity of these reactions has also been noted (Feiling) the belief none the less persists that the post-operative syndrome is a specific one. Muncie (1934) presented four cases showing an unusual type of excitement post-operatively. The elation and excitement he regarded as compensatory for the fear and

panic of the operation and its associated procedures (injections, transfusions, etc.). These performed without adequate explanation, together with the effects of drugs, dehydration and fever, and the misinterpretation of the attentions of nurses and doctors were thought to have produced a confusional state in which fear was very much in evidence. McDermott and Cobb (1938) likewise described a reaction differing from "the usual post-operative delirium". States of dreaminess and confusion, with panic, ideas of persecution, and even delusions and hallucinations occurred in four cases. This syndrome according to these authors was representative of a condition common in general surgical practice. Others have thought a manic type of reaction to be characteristic (Washburne and Carns, 1935), while a clinical picture resembling a state of agitated depression has been regarded as typical in women after lower abdominal operations (Lindemann, 1941). Exaggerated affective responses to operation have been described by V. H. Rosen (1950) in four patients following the loss of important body parts or functions. Although depressive symptoms more usually occurred, the manic episodes that were seen on occasion were believed to serve the purpose of denying the reality of the patient's situation.

A series of cases reported from a mental hospital (Oltman and Friedman, 1943) is of particular relevance here. Whereas the elderly patients presented with confusion and disorientation, the younger ones—aged under forty-five—suffered from a condition "usually quite indistinguishable from the ordinary case of schizophrenia". The authors concluded that any of the usual psychiatric entities may occur; they should be regarded as "post-operative" only in so far as the operation served as the exciting factor.

THE PRESENT STUDY

It was the purpose of this investigation to re-examine the problem of the post-operative psychoses, with special consideration of their clinical manifestations, their incidence, the types of surgical operations involved and of other aetiological factors.

The case material. A patient was regarded as suffering from post-operative psychosis if a mental disorder classifiable as psychotic had been precipitated by an operation. Eighty patients falling into this category were observed within a five-year period, 69 of them at St. Francis' Hospital Observation Ward and the remainder at the Maudsley Hospital nearby. The difficulties in controlling such material are considerable. Observation ward patients may be characterized by the severity of their behaviour disorder as well as by the social class from which they derive. The number of "specialist" hospitals in the "catchment area" may also influence the sample, for if an observation ward is close to such a hospital it will be likely to have an undue proportion of individuals suffering from psychosis after one type of operation (i.e. eye, gynaecological, etc.). If there was a complete lack of such hospitals in the area the sample could be equally unrepresentative by having too few cases of this kind. A survey of the hospitals in which the relevant cases had been operated on was performed. It justified the assumption that the sample was fairly representative of post-operative psychoses. The patients had come from thirty-five hospitals in all parts of London and even beyond, including all the London teaching hospitals with their active special departments.

We had no means of estimating the number of patients "lost" by direct transfer from a surgical department to a mental hospital or nursing home. Such factors as non-availability of observation ward beds, class of patient, existence of psychiatric facilities within the hospital in which the operation was carried out, ought to be taken into account. This was not possible, which makes the representative character of our sample somewhat uncertain. However, there were no indications that the leakage from these sources was considerable. Post-operative psychoses are mental disorders which develop suddenly, and in London patients of this type are admitted to the observation wards whenever possible.

Sites of operations. The sample comprised 57 females and 23 males aged 18 to 84. Their operations had been as follows:

	<i>Operations</i>							<i>M</i>	<i>F</i>
Eye								—	2
Nose .. .								2	—
Teeth .. .								2	7
Thyroid .. .								—	5
Breast .. .								—	3
Heart .. .								1	—
Stomach .. .								4	1
Appendix .. .								1	6
Hernia .. .								4	4
Bladder .. .								3	1
Gynaecological .. .								—	22
Other abdominal .. .								2	3
Orthopaedic .. .								2	4
Varicose veins .. .								1	—

The most striking features of this list are the predominance of females and the high proportion of gynaecological operations. The significance of these figures is to be discussed later in this paper.

In the reports received from the surgeons there was no evidence of serious operative complications in any of those patients. The absence of such complications was confirmed in fifty cases by one of us (B.B.Z.) who examined the original full ward notes. Nor were references to untoward incidents related to the use of anaesthetics found, except in the case of a patient who developed ether convulsions halfway through a hysterectomy which proceeded uneventfully after the convulsions had subsided under pentothal. The psychosis commenced several weeks later.

Minor post-operative complications (stitch abscess, haematoma, urinary infections, mild pulmonary infections) were present in six patients. They promptly subsided with chemotherapy and local measures, and appeared unrelated with the post-operative psychosis. In only three patients were there more serious post-operative complications which appeared to coincide with the onset of the psychosis: a woman, aet. 41, had a haemorrhage into the vagina ten days after a gynaecological operation. She was given a blood transfusion following which she became very talkative and developed a manic psychosis. Another woman, aet. 31, had a pyrexia up to 102° three days after a Fothergill operation for prolapse. A *Bact. coli* urinary infection was found and successfully treated with streptomycin. She was re-admitted several days later with a slight vaginal haemorrhage and with difficulties of micturition. Now she was depressed and heard people telling her to drink large amounts of water. A typical depressive illness lasting several months ensued. The third serious post-operative surgical complication encountered was an infected haematoma of a thyroidectomy wound which preceded a confusional state. Whether or not in these patients the mental disorder would have developed without the intervention of the particular complications it is impossible to say. It does not appear, however, that the incidence of such complications in the group under review was greater than would be expected in any comparable sample.

Reaction types of post-operative mental disorder. The following reaction types were represented among the patients observed:

Affective illness 46 (depression 36, mania 9, mixed state 1)

Schizophrenia 12

Confusional states 21 (of these, 8 were associated with dementia and 1 with heart failure)

Neurotic reaction 1

Twelve of the 21 patients who developed a confusional state had been free from psychiatric symptoms at the time of the operation, while 8 patients were on retrospective examination found to have been demented before, though manageable at home. In those patients and in the one with heart failure the confusional episodes occurred immediately after the operation or on the following day, but of the other psychoses only two followed within 36 hours of the operation, while the remainder had a latent interval of from 6 to 14 days.

The fact that neurotic reactions were observed in one case only is entirely due to the fact that neurotics are only rarely admitted to an observation ward.

Intervals between operation and onset of psychiatric symptoms. The interval in the cases where this could be established were as shown in Table I.

TABLE I

Time from operation	0-24 hours	-3 days	-5 days	-10 days	-2 weeks	-5 weeks	-9 weeks
Number of patients	7	7	8	15	12	13	3

Apart from the early appearance of confusional symptoms mentioned above, there was no relationship between interval and type of operation.

Symptoms. They conformed on the whole to those of the reaction types observed generally. An admixture of features of a confusional state, especially of a degree of perplexity, to an otherwise typical psychosis was not infrequent.

Four cases showed a mixture of confusion with other symptoms in the early acute stages, which subsequently cleared to give the more characteristic picture of one or other of the usual reaction types. The following two cases exemplify this course of events.

Male, 39. Quite well until one week after dental extraction under "gas" when he had had a vision of the Lord standing by him. He later became restless and confused and talked of the Lord and Lord's Day. From the Observation Ward where he presented a picture of a confusional state with Ganser-like features, he went to a mental hospital. There he deteriorated. His delusions, hallucinations and thinking were characteristic of schizophrenia. His violent behaviour necessitated certification. Three years later he was still in hospital.

Female, 46. No previous mental illness until 6 days after operation for hernia. Then she became acutely depressed and disorientated for time, place and person. The confusion cleared but her delusions persisted and she heard critical voices and felt unclean, guilty and ruined. This depressive state responded to a course of E.C.T.

In some patients the news of the impending operation seemed to have had a disturbing effect. A woman of 45 became very anxious on hearing that she needed a hysterectomy. Three weeks after operation she developed a severe depressive reaction requiring hospital treatment. Another patient expressed a dread of the anaesthetic when she learnt that she was to have a ventrisuspension operation. She feared she might die during the operation which was followed by a depressive illness.

The significance of these emotional reactions preceding the operation is uncertain. They are very common but only exceptionally are they followed by post-operative mental disorder.

Psychiatric sequelae to successive operations. There were no consistent reactions to successive operations in individual patients. In some cases each surgical intervention was followed by a psychotic reaction; but more usually while one operation has this result, others both before and afterwards have no such sequel.

A woman of 35 who became hypomanic after a varicose vein operation earlier in the year, suffered a more severe manic reaction after mastectomy later.

Another woman, aet. 57, whose vaginal repair in 1952 was followed by a post-operative psychosis, had had operations previously without complications: hysterectomy, 1922; appendicectomy, 1935; and mastectomy, 1938.

Woman, aet. 47; colostomy operation was followed by an acute affective illness, but three subsequent operations: bowel excision, closure of colostomy and salpingo-oophorectomy were all without sequelae.

Post-operative and puerperal psychoses in the same patient. The present study would appear to confirm the similarities between these two groups of psychoses which have been pointed out by previous authors. There were three patients in this series who had at some time previously developed an acute mental illness following childbirth.

A woman whose first puerperal mania followed the birth of her fifth child at the age of 30, had a sixth without mishap two years later. A hysterectomy at 49 was also uneventful, but a colpo-perineorrhaphy at 64 led to a post-operative depressive psychosis.

Female, aet. 40. Five confinements had been uneventful as was a Fothergill operation a year before present illness. This, an agitated depression, occurred four weeks after the birth of sixth child by Caesarean section in which sterilization also was performed. In this case the precipitating event was an operation and childbirth at the same time.

A woman who had had a puerperal depression relieved by E.C.T., had a further attack after myomectomy 4 months later.

A woman of 31 was admitted for an acute attack of mania which started 13 days after bilateral salpingectomy. She had had four previous manic attacks, two of which had followed childbirth.

Location of operation related to post-operative psychosis. An attempt has been made to relate locations of operations to types of mental disorder and compare the groupings with control samples. Women alone have been considered because the number of observations we had on men was too small for any detailed statistical analysis. Patients under twenty were excluded because it was felt likely that many adolescents would find provision other than in an observation ward; while those over fifty-nine were not included so as to avoid possible senile complications. The type of reaction was classified as above, and at the same time cases were considered according to the location of the operation, and under the age groups shown (Table II).

It would seem from the table that a remarkable number of psychotic illnesses may occur after gynaecological operations. This would at first thought appear to bear out the contention that women are more prone to breakdown after these operations than after other kinds. A statistical test of this contention will be made later.

The small number of schizophrenic illnesses after operation is also notable, as is the high frequency of affective reactions.

Although the aims of this study must necessarily be limited by the lack of fully controlled data, it is still possible to pose certain questions and to try to answer them. For example, by comparing an estimate of the frequency of different types of operation in patients living in the area of our observation ward, with that of psychoses after these operations in the series, some idea can perhaps

TABLE II
Female Post-operative Psychoses

Age group	..	..	..	20-29			30-39			40-49			50-59			Total		
				Aff.	Sch.	Oth.	Aff.	Sch.	Oth.	Aff.	Sch.	Oth.	Aff.	Sch.	Oth.	Aff.	Sch.	Oth.
Type of post-operative psychosis (P.O.P.)	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Gynaecological	..	..	..	5			2+(2)	1	5+(2)	(1)			1	1	12+(4)1+(1)	2	20	
Abdominal (non-gynaecological)	..	2	1				3	(1)					4	(1)	9	1+(2)	-	12
Eye	..	..	..												1+(1)	-	1+(1)	2
Thyroid	..	..	..						1		3	1			2	-	3	5
Other operations	..	..	..				5+(1)	1					2		7+(1)	-	1	9
																35	5	8
																		48

(Figures in brackets indicate cases having had previous psychiatric treatment.)
Aff.: Affective Psychosis. Sch.: Schizophrenia. Oth.: Other psychoses.

be gained about whether operations on certain organs or body areas are more liable than others in causing psychotic illness. Another question within the scope of this study is whether there is any change of proneness to mental illness with age. This can be answered by comparing an estimate of the numbers of operations in the same population, with the frequency of the post-operative psychoses at each age level.

In order to attempt an answer to these questions, a sample was gathered to provide an estimate of the number of operations carried out upon different parts of the body at different ages. It consisted of all operations performed in one year on patients between the ages of 20 and 59 at two large general hospitals in the immediate neighbourhood of the observation ward, one of them a teaching hospital. The total number of cases in this control sample was as shown in Table III.

TABLE III

						Male	Female
Hospital I	..	..	..	..	..	441	965
Hospital II	..	..	..	..	..	1,098	2,391
Total	..	..	..	..	..	1,539	3,356

For the purpose of this investigation these cases have been summed; and while its representative nature may be questioned, we can only state that we know of no particular bias in the data.

A comparison of the frequency of male to female post-operative psychoses (P.O.P.). The relative proneness of males and females to post-operative mental disorder was estimated by comparing the frequencies of the post-operative psychoses with those of operations carried out on males and females over the same age range in the control sample from the two hospitals (Table IV).

TABLE IV

						Male	Female
Post-operative psychoses	..	..	..	..	..	24	48
Operations in control sample	..	..	..	..	..	1,539	3,346

A χ^2 test is not significant and this suggests that neither sex is the more prone to mental illness after operation.

It was further of interest to discover whether the male:female ratio in the post-operative psychoses is the same as that occurring in psychoses generally. Table V below compares the post-operative psychoses with an example of psychotics as seen in the observation ward and mental hospital.

TABLE V

						Male	Female
Post-operative psychoses	..	..	..	..	..	24	48
Observation ward (admissions 1 year)	..	..	..	..	..	372	506
Mental hospital (1 year admissions)	..	..	..	..	..	168	306

(a) Between P.O.P. and O.W. samples χ^2 is not significant.

(b) Between P.O.P. and M.H. samples χ^2 is not significant.

(c) Between O.W. and M.H. samples $\chi^2 = 6.10$. This is significant at the .02 level.

There is therefore no evidence from the above Table to suggest that there is a difference in the proportion of males and females in either the post-operative or the control groups. The differences between these proportions in the two controls, while emphasizing the incompleteness of our sampling methods, need not invalidate the results of comparing each control with the P.O.P. sample.

A comparison of the frequencies of post-operative psychoses (P.O.P.) in women at different age levels. If the frequencies of operations at different age-levels in the control sample are assumed to be representative of the operations from which the sample of post-operative psychoses has been derived, then an estimate of change with age in proneness to post-operative psychoses may be obtained from Table VI below. This shows the frequencies of the post-operative and control samples in four age categories.

TABLE VI

Age Level				Post-operative Psychoses P.O.P.	Control Sample C.	P.O.P./C.
20-29	..	..	..	8	942	·008
30-39	..	..	..	12	1,022	·012
40-49	..	..	..	16	830	·019
50-59	..	..	..	12	562	·021
				48	3,356	·0143

A test of significance between the proportions at different age levels shows $\chi^2=7\cdot20$. This is significant at a $\cdot05$ level on a one-tail test.

If the assumption concerning the representative nature of the control sample is justified, then the ratio P.O.P./C. can be used as an indicator of the change with age of proneness to psychotic illness. This ratio increases with age. The χ^2 test suggests that this is a significant co-variation. Thus we may conclude that there is some evidence here to suggest that women become more prone to post-operative mental illness as they grow older. Is this a characteristic of the post-operative psychosis, or is it but an expression of a tendency found in psychoses generally? In Table VII the change in proneness to mental illness with age in the post-operative psychoses is compared with that of cases in the observation ward, and in a mental hospital.

TABLE VII

				Post-operative Psychoses	Mental Hospital	Observation Ward
20-29	..	..	..	8	54	103
30-39	..	..	..	12	65	141
40-49	..	..	..	16	101	151
50-59	..	..	..	12	86	111

(a) Between P.O.P. and O.W. sample χ^2 is not significant.

(b) Between P.O.P. and M.H. sample χ^2 is not significant.

(c) Between O.W. and M.H. samples $\chi^2=7\cdot63$. This is significant at the $\cdot05$ level on a one-tail test.

Table VII therefore shows that the post-operative sample reflects the same age tendency as in the other two, although there are differences on this score between the mental hospital and the observation ward samples.

An estimate of the relative proneness of women to post-operative psychosis after operations on different parts of the body. It has been maintained that a

person is more prone to psychosis after an abdominal operation than after surgery upon other parts of the body. A test of this can be made by comparing the frequencies of psychosis after abdominal and other operations with the frequencies of these types of operation in the control sample. This is done for different age levels in Table VIII.

TABLE VIII

	20-29		30-39		40-49		50-59		Total	
	Ab-dominal	Others	Ab-dominal	Others	Ab-dominal	Others	Ab-dominal	Others	Ab-dominal	Others
Post-operative sample	8	0	5	7	12	4	7	5	32	16
Control sample ..	604	338	702	320	531	299	330	232	2,167	1,189
	χ^2 not sig.		χ^2 not sig.		χ^2 not sig.		χ^2 not sig.		χ^2 not sig.	

From this Table it is clear that there is no evidence to suggest that abdominal operations are any more prone to cause psychosis than are operations on other parts of the body.

All gynaecological operations. Another strongly held view is that women are especially prone to psychotic illness after gynaecological operations. Table IX gives a comparison between psychoses after these operations and after non-gynaecological abdominal ones.

TABLE IX

	20-29		30-39		40-49		50-59		Total	
	Gynae-cological	Other Ab-dominal	Gynae-cological	Other Ab-dominal	Gynae-cological	Other Ab-dominal	Gynae-cological	Other Ab-dominal	Gynae-cological	Other Ab-dominal
Post-operative sample	5	3	5	0	8	4	2	5	20	12
Control sample ..	451	153	529	173	323	208	136	194	1,439	728
	χ^2 not sig.		χ^2 not sig.		χ^2 not sig.		χ^2 not sig.		χ^2 not sig.	

Here again there is no evidence to suggest a difference of proneness to psychosis after gynaecological operations than after others. The high frequency of psychoses after gynaecological surgery was matched by the large number of such operations performed in the area.

However, this held true only if all types of gynaecological operations were included in the control group. If operations were divided into hysterectomies and others, the result of the comparison was different.

A comparison of psychoses after hysterectomies and after other gynaecological operations. All age levels have been combined as the frequencies do not allow of a further breakdown. The result of the comparison is shown in Table X. Although the numbers are small the figures strongly suggest that hysterectomies are more liable to be followed by psychoses than other gynaecological operations.

TABLE X

	Hysterectomies				Other Gynaecological Operations	
	..	..	..	..	..	..
Post-operative sample	..	..	..	7	13	
Control sample	..	..	..	174	1,265	
$\chi^2=9.04$ significant at the .005 level						

The average age of the patients subjected to hysterectomy was 48 at the time of operation, while it was 35 among those subjected to other gynaecological

operations. The age distribution in the teaching hospital control group of hysterectomies showed a peak in the 41-50 age group, which was in accordance with the average age of the patients who developed mental disorders following this operation.

The type of psychotic reaction occurring post-operatively. Another problem is whether the post-operative case tends to develop any particular type of psychosis. This can be investigated by comparing the frequencies of the different types of post-operative psychosis in the series, with the frequency of the reaction-types found among a representative sample of psychotic patients.

The apparently small proportion of schizophrenics and large number of affective reactions in the post-operative psychoses has already been commented upon. An estimate as to whether these proportions differ significantly from what would be expected from the ordinary run of psychotic illnesses was obtained by comparing them with a control sample of other patients admitted in one year to the observation ward.

Table XI shows the frequencies of schizophrenic as opposed to other diagnoses in the post-operative group and control sample for women between the ages of 20 and 59.

TABLE XI

	20-29		30-39		40-49		50-59		Total	
	Schizo-phrenic	Others	Schizo-phrenic	Others	Schizo-phrenic	Others	Schizo-phrenic	Others	Schizo-phrenic	Others
Post-operative psychosis ..	1	7	0	12	2	14	2	10	5	43
Control sample ..	47	56	55	86	63	88	35	76	200	306
	$\chi^2=4.82$ Sig. at .05 level		$\chi^2=5.72$ Sig. at .01 level		$\chi^2=5.90$ Sig. at .01 level		χ^2 not sig.		$\chi^2=153.0$ Sig. at .0001 level	

Here the frequency of post-operative schizophrenias is significantly less than would be expected if the post-operative patient was as prone to develop the different psychotic reactions as does the ordinary patient admitted to the observation ward. A person, therefore, appears to be unlikely to develop a schizophrenic reaction after an operation. Even if allowance is made for uncertainties of diagnosis, the level of significance in the above Table would still seem to validate this claim, provided the post-operative sample is representative.

The high preponderance of affective reactions among the post-operative patients also requires further scrutiny to decide whether the differences between the affective and other reactions differ from the general distribution of psychotic breakdowns. In Table XII all schizophrenic patients are excluded, and a comparison is made between the frequency of affective and other reactions in the post-operative group, and the control sample of observation ward patients. These are once again all women between the ages of 20 and 59.

TABLE XII

	20-29		30-39		40-49		50-59		Total	
	Affective	Others	Affective	Others	Affective	Others	Affective	Others	Affective	Others
Post-operative psychosis ..	7	0	10	2	11	3	7	3	35	8
Control sample ..	38	18	59	27	65	23	57	19	219	87
	χ^2 not sig.		χ^2 not sig.		χ^2 not sig.		χ^2 not sig.		χ^2 not sig.	

From this Table it would appear that post-operative reactions are no more likely to be affective in type than in psychoses generally. We can also confirm

from this Table that the earlier findings about increased proneness with age (Table XII) applies also to the affective reactions.

The effect of location of operation upon the type of psychotic reaction. The suggestion is made from time to time that abdominal and gynaecological operations are more likely to evoke affective rather than other types of reaction.

Table XIII gives some evidence concerning this question. It shows the frequencies of schizophrenic, affective and other reactions to gynaecological, abdominal and other operations in the sample of female post-operative psychotics. All ages between 20 and 59 are pooled, as the numbers are too small to consider otherwise.

TABLE XIII							
					Schizophrenic	Affective	Others
Gynaecological	..	..	..	..	2	16	2
Abdominal (non-gynaecological)	..			..	3	9	0
Others	..	..	..	..	0	10	6

A break-down of this Table so that Fisher Yates Tests might be applied was carried out. The results suggested that neither gynaecological nor other abdominal operations are more likely to produce affective reactions than are operations on any other part of the body.

DISCUSSION

The scope of this study was limited by the nature of the clinical material available. The large majority of the patients were observed in a mental observation ward where only patients with serious behaviour disorder are admitted. For this reason milder post-operative psychiatric disorders, especially those of a neurotic type which are probably much more frequent than those reported here, were not included.

There was no evidence that there is an entity "post-operative psychosis". The clinical status of "post-operative psychosis" is similar to that of "puerperal psychosis", which likewise has been proved to manifest itself in a variety of common reaction types (Jacobs, 1943). Whether and what type of psychosis develops in an individual obviously depends on predisposing factors, the operation or childbirth respectively acting as the precipitating cause. The role of emotional factors, which has been assumed to be important for both, remains uncertain, at least as far as the post-operative psychoses are concerned. The fact that several patients of this series had had puerperal as well as post-operative psychiatric illnesses demonstrates the kindred nature of these conditions. They also have in common the frequent feature of an interval between the stressful event and the onset of manifest psychiatric symptoms.

In none of the patients of this series could anaesthesia be regarded as an aetiological factor. Such an assumption would be justified only in cases where no mental disorder had been present before the operation and where there was a history of prolonged anoxia during the operations. In none of the patients of this series was there such a history.

An attempt has been made to examine the question whether there is a significant correlation between the site of the operation and the incidence of post-operative psychoses. No such relationship could be established except for hysterectomies, which seemed to carry a greater risk of psychotic illness than other gynaecological operations.

The reasons for the comparatively higher incidence of psychoses following hysterectomy could not be fully explored in this investigation, as a closer psychological study of individual patients was not possible. Several factors may contribute to this exceptional position of hysterectomy among gynaecological operations: its psychological significance, its frequency in a particularly vulnerable age and the magnitude of the surgical procedure involved. The importance of the age factor could be clearly demonstrated.

Possibly other operations might have been found to play a similar role to that of hysterectomy in precipitating mental disorders, had the size of our sample been greater. Our investigation therefore, has not invalidated statements to that effect, although it has made them more questionable. It has pointed to the way in which they could be tested. The final answer might come from large scale investigations carried out on a regional basis; only thus could the representative character of the sample of post-operative mental disorders and of the controls be ensured.

The comparatively large proportion of dental operations in this series is noteworthy. This is not surprising considering the enormous incidence of these operations, and it can be assumed that they acted, like the other operations, as precipitating events in predisposed individuals. It is, however, of interest that they had in this respect the same effect as the other operations, most of which belonged to the field of major visceral surgery. In all cases concerned the dental operations consisted in multiple extractions carried out either in one session or in quick succession.

In view of the finding that the incidence of psychoses following operations did not exceed expectation, the question may be asked whether in the cases observed the association of psychosis with the operation was not purely coincidental. One of the reasons why this is unlikely is the frequent occurrence of perplexity and bewilderment in the clinical picture, a feature indicative of an organic origin.

Would those patients, then, not have developed a mental disorder if they had not had their operations? The answer is that they probably would not have had that mental illness at that particular time, but probably on some other occasion and precipitated by some other stressful event physical or mental. Apparently, a certain proportion of psychotic illness in general is reactive in that sense, and operations represent one group of the agencies precipitating psychoses in those predisposed to them. It seems likely that childbirth acts in the same way. The finding, therefore, that the incidence of psychoses following operations did on the whole, not exceed expectation, does not imply that the relationship between the two events is coincidental.

The fact that the number of post-operative schizophrenic psychoses was even lower than could be expected in a corresponding number of patients admitted to a mental observation ward may have been due to the character of the sample which possibly was not quite representative. However, it is more likely to be due to the nature of schizophrenic reaction types, whose onset is usually gradual and insidious and which one would therefore expect to be under-represented among a group of patients whose mental disorders have a circumscribed precipitating event and an acute onset in common.

CONCLUSIONS

A series of 80 cases of post-operative mental disorder were studied from the clinical and statistical points of view.

A variety of common reaction types was observed. Confusional states

related solely to the operation, formed only a small group. There was no evidence for the existence of a clinical entity "post-operative psychosis". The largest single group among the reaction types observed was that of affective illness. There was no indication that the surgical complications or the use of anaesthetics were of aetiological importance.

The types of post-operative mental disorders observed were similar to those occurring after childbirth. Post-operative and puerperal psychoses share the common feature of an interval between the stressful event and the onset of psychiatric symptoms. Some patients had had both post-operative and puerperal psychosis.

Abdominal and especially gynaecological operations were highly represented in the sample studied. A comparison of the relative frequency of such operations among control groups demonstrated that their high representation among the post-operative psychoses was matched by their high incidence among the general population. Hysterectomy was followed by psychosis comparatively more frequently than other gynaecological operations. Age could be shown to be an important factor in this discrepancy.

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REFERENCES

- ABELES, M. M., *Amer. J. Psychiatry*, 1938, **94**.
 ALI, Z., *Arch. Suisse de Neurol. et Psych.*, 1941, **47**, 1.
 BAKWIN, H. J., *Paediat.*, 1952, **36**, 262.
 BARNES, F., *Surg. Gynaec. Obstet.*, 1916, **22**, 579.
 BATTEN, C. T., and COURVILLE, C. B., *Anaesthesiology*, 1940, **1**, 261.
 COBB, S., and McDERMOTT, N. T., *Med. Clin. N. America*, 1938, **22**, 569.
 COLLINS, W. J., *Lancet*, 1888, December 15, 1175.
 DA COSTA, J. C., *Surg. Gynaec. Obstet.*, 1910, **11**, 572.
 DENT, C. T., *J. Ment. Sci.*, 1889, **25**, 1.
 DEUTSCH, H., *Psychosom. Med.*, 1942, **4**, 105.
 DOYLE, J. B., *Brit. J. Anaesth.*, 1938, **6**, 37.
 DUPUYTREN, —, *Lancet*, 1833, Vol. 11.
 EBAUGH, F. G., *Bull. Amer. Coll. Surg.*, 1937, **22**, No. 3.
 FEILING, A., *Practitioner*, 1937, **138**, 1259.
 GREENWOOD, A., *J.A.M.A.*, 1928, **91**, 1713.
 HOBBS, A. T., *Brit. Med. J.*, 1897, **11**, 769.
 JACOBS, B., *J. Ment. Sci.*, 1943, **89**, 242.
 KEITH, —, quoted by G. B. Bantock, *Brit. Med. J.*, 1889, 7 August.
 KELLY, H. A., *Amer. J. Obstet. Gynec.*, 1909, **59**, 1035.
 LINDEMANN, E., *Amer. J. Psychiat.*, 1941, 98–132.
 LEWIS, A., *Report XVI Congrès International de Chirurgie*, 1955.
 MENNINGER, K. A., *Psy. Quart.*, 1934, 111.
 MEYER, A., *Recent Progress in Psychiatry*, 1944, Vol. 1.
 MUNCIE, W., *Arch. Neurol. and Psych.*, 1934, **32**, 4.
 OLTMAN, J. E., and FRIEDMAN, S., *Psychiat. Quart.*, 1934, **17**, 3.
 PREU, W. P., and GUIDA, F. P., *Arch. Neurol. and Psychiat.*, 1937, **38**, 818.
 ROHE, G. H., *Brit. Med. J.*, 1897, **11**, 766.
 ROSEN, V. H., *Psychosom. Med.*, 1950, **12**, 356.
 RUSSELL, J., *Brit. Med. J.*, 1897, **11**, 770.
 SAVAGE, G. H., *Brit. Med. J.*, 1887, 3 December.
 TAIT, L., *Brit. Med. J.*, 1889, 31 August.
 WASHBURNE, A. C., and CARNS, M. L., *J. Nerv. and Ment. Dis.*, 1935, **82**, 5.

AN EVALUATION OF PREFRONTAL LEUCOTOMY IN THE AFFECTIVE DISORDERS OF OLD AGE: A FOLLOW-UP STUDY

By

F. T. THORPE, M.R.C.S., L.R.C.P., D.P.M.

*Medical Superintendent
Middlewood Hospital, Sheffield*

THE senile emotional disorders are now a major problem in mental hospital practice and their incidence increases with age. It is fortunate, however, that a better understanding and recognition of these affective disorders has, in most cases, led to early treatment and recovery.

A detailed study of the various mental disorders of the senescence has shown that functional disorder is more common than had previously been suspected, and it is probable that one half of patients over 60 years of age admitted to a mental hospital have a recoverable melancholic state, the remainder consisting mostly of the more serious organic psychoses (Roth, 1952). Many of the emotional breakdowns occur for the first time in old age, and their rapid onset with a depressive mood serves to distinguish them from senile cerebral degeneration with its gradual intellectual impairment. There is little overlap between these two conditions although the differential diagnosis may be difficult when apathy is the dominating feature of the case.

In doubtful cases of functional disorder, the diagnosis can often be settled by giving three or four applications of electroplexy as a therapeutic test which does little harm in organic cases. Indeed, the beneficial effects of E.C.T. in senile melancholia have long been established. A discharge rate of 75 per cent. can be obtained (Ehrenberg *et al.*, 1955), but as many as 37 per cent. may relapse, and although further E.C.T. is of benefit, a modified lower quadrant prefrontal leucotomy will produce a more lasting recovery.

Prefrontal leucotomy is indicated in the recurrent or relapsing case of senile melancholia, or when E.C.T. fails or is contra-indicated on physical grounds. Such patients are faced with the prospect of indefinite confinement to the mental hospital, possibly for life, and there is a heavy toll in death from exhaustion, pneumonia and other intercurrent infections (Roth, 1952).

The value of any new treatment is best seen in the long-term follow-up, yet only a few reports of leucotomy in the aged have appeared, and this is a field which has still to be explored (Sargant and Slater, 1954). We therefore decided to review the outcome of the first 50 patients over the age of 65 submitted to leucotomy at Middlewood Hospital during the period 1947 to 1952.

A recent follow-up has been made on the 50 cases and a report obtained for every patient. A questionnaire was sent to the next of kin of patients discharged, and nine of these were followed up by personal interview. By these means an assessment has been made of both the early and the late results of the operation, the latter giving the present condition of 32 patients still living five to nine years after leucotomy.

The results of these assessments are shown in the tables under three grades of post-operative improvement. A full remission or recovery (+ +) indicates

an absence of anxiety, tension or depression, or any of the symptoms shown before operation, a return to the former level of social adaptability and an absence of any post-leucotomy personality defects or mental deterioration. A partial remission (+) implies that some improvement or mitigation of the emotional disorder has occurred, tension and depression lightened, with social improvement and less dependence on others. In some of these cases a mild degree of apathy together with reduced initiative persisted. The third category of no remission (0) means a return to the pre-operative level of psychiatric disability with little or no amelioration of symptoms. In no case apart from one death from post-operative haemorrhage could it be said that the patient was made worse or demented.

CASE MATERIAL

The selection of cases was determined by the severity of the symptoms, and the failure of E.C.T. to produce sufficient or a lasting improvement or when E.C.T. seemed inadvisable. In recent years the use of muscle relaxants has increased the safety of E.C.T., and extended its use in old people.

In 39 cases the clinical picture was that of agitated melancholia with restlessness, lamentations, suicidal trends, refusal of food and delusions of self-depreciation. In 7 cases there was apathy and mental retardation. There were 2 cases of senile paranoid psychosis and 2 cases of senile mania. Active suicidal tendencies were present in 17 cases.

None of the patients gave a history of progressive intellectual or memory loss. The duration of psychiatric illness varied from a few months in recurrent cases to several years in others, with no significant difference in outcome from operation. Case No. 6 for example, was in hospital for $7\frac{1}{2}$ years despite E.C.T. and was discharged after operation.

There were 6 men and 44 women in the group, their ages ranging from 65 to 74, with a mean age of 68. There was no association of age and recovery, and in three patients aged 74, two were discharged home. The number of patients with previous admissions to hospital was 25, and 15 of these had had attacks prior to the age of 60 years. There was no appreciable difference in the results of cases with early and late onset.

Acute and chronic physical illness is known to be common in the emotional disorders of old age (Roth *et al.*, 1956), and in this series care was taken to exclude any serious organic disease, but a moderate degree of hypertension in the absence of cerebral symptoms was accepted. The diastolic blood-pressure was over 100 mm. Hg in 13 cases, yet 9 were discharged home, and one died within two years of operation.

In all cases the psychiatric prognosis had been regarded as poor, and in 25 patients the response to E.C.T. was disappointing. In the other 25 cases E.C.T. was not given for varying reasons, mostly because of hypertension or tendencies to relapse. The operation results were similar in both groups.

OPERATIVE PROCEDURES

A standard prefrontal leucotomy (Freeman and Watts, 1944) was carried out in 6 cases (Nos. 1, 3, 4, 15, 42, 43) and in 42 cases the incision was restricted to the lower quadrants in the anterior plane of the prefrontal areas of the brain as described in a previous report (Thorpe and Hardman, 1952). In the last two cases (No. 49 and 50) an electrocoagulation of the ventromedial quadrants was performed according to the technique of Grantham (1951). All the operations

were performed by Mr. James Hardman in the Neurosurgical Department of the United Sheffield Hospitals. In every case a general anaesthetic was avoided.

Post-operative complications were few, but one patient (No. 16) died from cerebral haemorrhage on the 10th day and another (No. 42) developed an infection of the operation site which was successfully treated by an antibiotic. Post-operative epilepsy occurred in four cases and persisted in two (No. 2 and 15) though not to any serious degree.

EARLY RESULTS (UP TO 6 MONTHS)

The immediate results of the operation were excellent and are summarized in Table I. Only two patients failed to show a significant improvement—a

TABLE I
Early Results in Diagnostic Categories

Diagnosis	No. of Cases	Remission of Symptoms			Status at 6 Months		
		(++) Full	(+) Partial	(0) Nil	Dis- charged	Died	Still in Hospital
Agitated depression	39	30	8	1	26	1	12
Retarded depression	7	4	3	—	5	—	2
Mania	2	1	1	—	1	—	1
Paranoid	2	—	1	1	1	—	1
Total	50	35	13	2	33	1	16

paranoid patient and the one who died from operation. In all the other patients there was a noticeable relief of anxiety and tension, often seen by a ready smile a few hours after the operation. Agitated patients became quiet and relaxed, making no mention of their previous worries and lamentations. Mental confusion was rarely seen, though a transitory phase of "denial of operation" occurred in a few cases.

Patients are encouraged to return home as soon as possible and most of the 33 discharges left hospital within 2 months of the operation. A good remission of symptoms occurred in 35 cases (Table I) and a partial remission in 13, but for social reasons not all of these patients could leave hospital. The agitated depressive patients did well, and of the 7 with retarded depression (Nos. 5, 6, 27, 32, 40, 44, 46) five were discharged. The results in the 2 manic (Nos. 15 and 43) and the 2 paranoid patients (Nos. 4 and 48) were as follows. One manic patient made a partial recovery but could not be discharged, and the other was discharged with a good remission, though re-admitted later. There was no improvement in paranoid Case 48, a deaf old lady with persecutory delusions, and a partial recovery occurred in paranoid Case 41, who became resigned to his hypochondriacal symptoms and was discharged home.

LATE RESULTS

At the present survey 5 to 9 years after operation, 32 patients still survive, and a total of 35 (70 per cent.) have lived for over 5 years. There are 18 patients at home, 14 in hospital and 18 have died (Tables III, IV, V). A full recovery has been maintained in 21 cases, a partial recovery in 10, and 1 case is unchanged (Table II).

TABLE II
Results of Present Survey 5 to 9 Years After Leucotomy

Diagnosis	No. of Cases	Remission of Symptoms			Present Status		
		(++) Full	(+) Partial	(0) Nil	Dis- charged	Died	Still in Hospital
Agitated depression	39	18	8	—	16	13	10
Retarded depression	7	2	1	—	1	4	2
Mania	2	1	—	—	—	1	1
Paranoid	2	—	1	1	1	—	1
Total	50	21	10	1	18	18	14

Relapses

A total of 7 patients were re-admitted after discharge from hospital, 4 with a history of recurrent attacks. In addition, Case 46 died from a suicidal attempt after being admitted to a general hospital. Of the 7 re-admissions, 4 were subsequently discharged again, one after a second leucotomy operation (Case 4) and of the 3 relapsed patients still in hospital, 2 have regained a good remission again. Only 4 patients (Nos. 4, 25, 32, 46) could be said to have had a genuine relapse, Case 4 having relapsed completely. The other 4 patients were re-admitted for minor symptoms. The period between discharge and relapse in these patients ranged from 6 months to 4½ years.

Recurrent Depressions

Of the 25 patients who had two or more attacks of depression prior to operation, 2 had a genuine relapse after operation (Nos. 4 and 32) and were discharged again, and the remainder kept well. Case 26, for example, had previous admissions in 1931, 1940, 1942, 1948 and 1949 (2) and has now remained well at home for 7 years. Case 29 had admissions in 1939, 1942, 1947, 1948 and 1949 and has also been home for 7 years. Case 42 with 5 admissions has been home for 6 years, and Case 32 also with 5 admissions lived at home for 2 years. These cases support the claim that leucotomy prevents the recurrence of depressions.

Deaths

At the present time a total of 18 patients have died (Table III), and

TABLE III
Patients Deceased

Case No.	Age at Operation	Date of Operation	Blood Pressure	Early Results	Dis-charge	Re-admitted	Late Results	Died	Survival Years to Death	Cause of Death
2 ..	66	Nov. 1947	165/85	++	No		+	Feb. 1950	2½	Cardiovascular
3 ..	69	Jan. 1948	150/80	++	Yes		++	Sept. 1955	7½	Cardiovascular
4 ..	74	{Jan. 1948 Nov. 1950}	200/100}	++	Yes	Yes	++	Dec. 1952	4	Cardiovascular
5 ..	67	Aug. 1948	145/100	++	Yes		++	April 1949	½	Pneumonia
6 ..	66	Aug. 1948	130/80	+	Yes		+	May 1952	3½	Cardiovascular
7 ..	65	Aug. 1948	200/110	++	No		+	Mch. 1951	2½	Cardiovascular
10 ..	65	Nov. 1948	160/90	++	Yes		++	Oct. 1952	4	Cardiovascular
12 ..	68	Feb. 1949	200/90	++	Yes		++	April 1956	7	Pneumonia
14 ..	72	May 1949	180/100	+	No		+	Mch. 1950	½	Pneumonia*
15 ..	68	May 1949	140/90	+	No		+	Feb. 1954	4½	Cardiovascular*
16 ..	73	June 1949	190/90	0	No		0	June 1949	10 days	Haemorrhage*
18 ..	67	Aug. 1949	200/100	++	Yes		++	July 1955	6	Cardiovascular
20 ..	72	Sept. 1949	210/115	++	Yes		++	Jan. 1953	3½	Cardiovascular
21 ..	74	Sept. 1949	170/110	++	Yes		++	Aug. 1951	2	Cardiovascular
31 ..	66	Dec. 1949	130/70	++	Yes		++	Jan. 1953	3	Cardiovascular
32 ..	70	Dec. 1949	200/110	++	Yes	Yes	+	Sept. 1951	1½	Cardiovascular
33 ..	68	Dec. 1949	150/80	++	Yes		+	April 1953	3½	Cardiovascular
46 ..	65	Nov. 1951	150/90	+	Yes		+	Feb. 1956	4½	Suicide*

* Autopsy.

excluding the post-operative death, the survival period ranged from 6 months to 7 years, the causes of death being unrelated to the operation. Death occurred at home in 8 cases; in the mental hospital, 5; and in general hospitals, 5. The causes of death were cardiovascular, 13; pneumonia, 3; suicide, 1; cerebral haemorrhage, 1; and were determined by autopsy in four cases, and the rest by reports from relatives.

The observations of relatives of patients who died after discharge from hospital are full of praise for the results of the operation, and it was apparent that in many cases the patient was enabled to live the remainder of life with serenity of mind. Replies to enquiries have been couched in such terms as "better in his nerves than he had been for a long time" (Case 5), "pleased with the results of the operation" (Case 4), "results entirely satisfactory" (Case 12), "the operation made a wonderful difference" (Case 20), "more alert" (Case 3), "mentally well up to the time of death" (Case 10).

Patients Still in Hospital

There are 14 patients still in hospital 5 to 8 years after operation although only one failed to improve (Table IV). Of the 7 patients with an early remission, 5 have remained well and could be discharged home or to hostel accommodation. The 8 partial remissions have some residual emotional defects, and four

TABLE IV
Patients Still in Hospital

Case No.	Age at Operation	Date of Operation	Blood Pressure	Early Results	Re-admitted	Late Results	Survival Years to Date
9 ..	67	November 1948	190/100	++		++	8
11 ..	66	November 1948	170/100	+		+	8
13 ..	66	May 1949	120/70	++	Yes	+	7
17 ..	66	July 1949	190/80	+		+	7
34 ..	65	February 1950	140/75	++	Yes	++	7
35 ..	68	March 1950	210/120	++		++	7
37 ..	65	July 1950	220/140	++		+	6
38 ..	66	July 1950	160/90	+		+	6
39 ..	68	August 1950	150/90	+		+	6
40 ..	66	August 1950	160/100	+		+	6
43 ..	66	December 1950	180/100	++	Yes	++	6
44 ..	69	May 1951	150/90	++		++	5
48 ..	72	March 1952	170/80	0		0	5
49 ..	74	March 1952	220/120	+		+	5

of them still need psychiatric care mainly because of apathy and lack of initiative (Cases 13, 17, 39 and 49). Case 48 with paranoid symptoms remains unchanged.

Patients Still at Home

There are 18 patients living at home 5 to 9 years after operation (Table V), 16 maintaining a full remission although 2 had to return to hospital during a short relapse of depressive symptoms. In Case 1, there was a history of 3 years depression and head noises, but she has now been at home for 9 years and is 80 years old. The head noises are still present but she is no longer depressed by them.

Sample reports from these patients and their relatives leave no doubt of the value of the operation, e.g. "a miracle", Case 19; "the operation has been a great success, she has been quite normal since returning home", Case 50;

"operation has been very successful", Case 45; "she is wonderful", Case 30; "completely successful, with gratitude and sincere thanks", Case 47.

TABLE V
Patients Still at Home

Case No.	Age at Operation	Date of Operation	Blood Pressure	Early Results	Re-admitted	Late Results	Survival Years to Date
1 ..	71	November 1947	170/100	++		+	9
8 ..	67	August 1948	160/90	++		++	7
19 ..	65	August 1948	170/100	++		++	7
22 ..	67	September 1949	200/100	++		++	7
23 ..	65	September 1949	170/110	++		++	7
24 ..	68	October 1949	160/90	++		++	7
25 ..	71	November 1949	170/100	++	Yes	++	7
26 ..	66	November 1949	210/115	++		++	7
27 ..	65	November 1949	190/110	++		++	7
28 ..	69	November 1949	180/115	++		++	7
29 ..	67	November 1949	190/90	++		++	7
30 ..	66	December 1949	160/90	++		++	7
36 ..	66	May 1950	170/100	++	Yes	++	6
41 ..	66	October 1950	170/90	+		+	6
42 ..	66	December 1950	150/90	+		++	6
45 ..	65	June 1951	220/110	++		++	5
47 ..	65	January 1952	150/90	++		++	5
50 ..	67	April 1952	190/120	++		++	5

Post-leucotomy Personality Defects

In this group of 50 elderly patients, post-operative personality defects have been absent. Such undesirable behaviour trends as inconsiderateness, irritability, volubility and emotional facility, reported after the more extensive operations, are less likely to occur following an anterior lower quadrant incision. No case developed a dementia, or the "loud laugh that speaks of the vacant mind". In Case 13, there was a short phase of obscene mutterings, and Case 42 went through a period of truculent mania. Most of our patients had good pre-psychotic personalities albeit characterized by over-conscientiousness and guilt-proneness which, while predisposing them to melancholic reactions, protected them from any excessive disinhibitory effects of leucotomy.

COMMENT

It is pertinent to consider what would have happened to these 50 patients if they had not had leucotomy. They were all considered to have a poor prognosis, either in relation to the current attack of mental illness, or the probability of relapse. Many had failed to benefit from E.C.T., and it is doubtful if any lasting benefit would have been obtained by the use of tranquillizing drugs.

It is likely that without operation their expectation of life was reduced, though the survival-rate in the senile depressive is higher than that in the organic dementias. Roth and Morrissey (1952) found that in a group of 81 patients over 60 years of age admitted to a mental hospital with emotional disorders, 23 per cent. were dead in 2 years. In another similar group of 56 cases admitted to an observation ward, Post (1951), reports 27 per cent. died in 3½ years. It is not possible, however, to compare these findings with our group of cases which were not only over 65 years of age, but were selected by exclusion of serious physical illness, and many had been in hospital for lengthy periods.

It has been reported in several papers that the most favourable results from leucotomy are to be found in the older patients, and our study confirms this.

The Board of Control (1947) in a survey of 1,000 leucotomy patients of all ages reports that 17 of the 36 patients over 65 (47.2 per cent.) were discharged and remained at home. Scoville (1955) reports the results of orbital undercutting in 20 patients over 65 years of age, with significant improvement in all, and discharge in 17 (88 per cent.).

The comparative rarity of relapses following leucotomy in the older age group was observed by Gillies (1952) and in our long-term follow-up only 4 cases (8 per cent.) had a genuine relapse. One of these was severe but recovered after a second operation, and no case had more than one relapse. Since 5 of the re-admissions had a history of recurrent attacks, it must be concluded that leucotomy reduces but may not entirely abolish the tendency to recurrence.

In our earlier paper (Thorpe and Hardman, 1952) we reported a discharge rate of 67 per cent. of patients aged over 60 (25 of 43 cases) and some of these form part of the 66 per cent. discharges in the present survey. At the time of writing there are 18 patients still at home 5 to 9 years after operation, and if we add to these the 13 patients still in hospital though improved, a total of 31 patients (62 per cent.) have so far maintained improvement for at least 5 years.

In the selection of cases suitable for leucotomy, one would expect the best results when organic cerebral disease is minimal, and the clinical history, psychiatric findings and physical examination give a clear picture of functional disorder. Cases with marked arteriosclerosis, a blood urea of over 50 mg. per cent. or a cerebrospinal fluid protein of over 60 mg. per cent. require careful assessment before undertaking a leucotomy. It is to be anticipated that cerebral arteriosclerosis will increase the risk of post-operative haemorrhage, but a moderate degree of hypertension is quite compatible with a good post-operative recovery.

None of our 13 cases with diastolic blood pressures of over 100 mm. Hg. developed a senile dotage, and most of them made a good recovery, though there was no evidence of any lasting change in the blood pressure readings. It has been reported, however (Lenstock and Groen, 1953), that leucotomy was beneficial in a case of malignant hypertension with psychogenic symptoms.

Post-operative epilepsy is an accepted though not serious risk, and is usually readily controlled by anticonvulsants. Of our 4 cases, 2 developed an early fit 4 and 11 days after operation (Cases 12 and 30) and both made a full recovery with no further fits. The other two cases (Cases 2 and 15) however, developed late fits 5 and 11 months after operation, and here the results were poor, thus confirming the observations of Stengel (1950) and Meyer and Beck (1954) that these cases usually do badly. An autopsy on Case 15 revealed that the full leucotomy incision had involved the cerebral cortex to a considerable degree—a feature found by Meyer and Beck (1954) to predominate in the epileptic leucotomized patients.

The rarity of undesirable post-leucotomy personality defects in the elderly patient is confirmed in this study, and the only after-effect observed was the occasional occurrence of apathy, lassitude and diminished initiative. Our results confirm the opinion of Scoville (1955) that there is little risk of mental deterioration arising after a limited or modified leucotomy operation in the non-organic mental disorders of the aged, and that it is the treatment of choice whenever the patient fails to respond to one course of E.C.T.

The lasting benefit from intolerable anxiety and depression speaks for itself, and will be seen in the follow-up reports from patients and their relatives. The incidence of relapses will be small, and most patients will be able to enjoy their remaining years at home.

SUMMARY

A long-term follow-up has been made on the results of a limited leucotomy in a series of 50 elderly patients over 65 years of age admitted to a mental hospital with emotional disorder, and an assessment made 5 to 9 years after operation. The clinical diagnoses were agitated depression 39; retarded depression 7; paranoid state 2; mania 2. Recurrent attacks occurred in 25 cases prior to operation.

All patients except two showed appreciable improvement following the operation, 35 with complete and 13 with partial remission of symptoms. Discharges number 33. There was one death from post-operative haemorrhage, and 4 patients developed post-operative epilepsy.

At the present assessment, 32 patients are still alive, 18 at home and 14 still in hospital. Of these, 21 (16 at home) are fully recovered, 10 (2 at home) partially recovered and one unchanged. Within a period of eight years 18 patients have died—16 from natural causes. Only 4 patients relapsed after returning home.

It is concluded that a lower quadrant leucotomy is an effective treatment for intractable or recurrent senile melancholic states. At least two-thirds will recover completely and survive for at least five years without relapse. There is little danger of post-operative mental deterioration, and most patients will be enabled to live the remainder of their lives with added years of equanimity.

REFERENCES

- BOARD OF CONTROL, *Pre-Frontal Leucotomy in 1,000 cases*, 1947.
 EHRENBERG, R., *et al.*, *Amer. J. Psychiat.*, 1955, **111**, 743.
 FREEMAN, W., and WATTS, J. W., *J. Ment. Sci.*, 1944, **90**, 532.
 GILLIES, H., *et al.*, *Brit. med. J.*, 1952, *i*, 527.
 GRANTHAM, E. C., *J. Neurosurg.*, 1951, **8**, 409.
 LENSTOCK, C. H., and GROEN, I., *Rev. Neurol.*, 1953, **89**, 536.
 MEYER, A., and BECK, E., *Pre-frontal Leucotomy and Related Operations*, 1954. London: Oliver and Boyd.
 PARTRIDGE, M., *Prefrontal Leucotomy*, 1950. Oxford: Blackwell.
 POST, F., *Brit. med. J.*, 1951, *i*, 436.
 ROTH, M., and MORRISSEN, J. D., *J. Ment. Sci.*, 1952, **98**, 66.
Idem and KAY, D. W. K., *J. Ment. Sci.*, 1956, **102**, 141.
 SARGANT, W., and SLATER, E., *Physical Methods of Treatment in Psychiatry*, 1954. 3rd edition. London: Livingstone.
 SCOVILLE, W. B., and RYAN, V. G., *Geriatrics*, 1955, **10**, 311.
 STENGEL, E., *J. Ment. Sci.*, 1950, **96**, 633.
 THORPE, F. T., and HARDMAN, J., *J. Ment. Sci.*, 1952, **98**, 389.

A CLINICAL AND PSYCHOMETRIC STUDY OF THE EFFECTS OF PROCAINE AMIDE IN HUNTINGTON'S CHOREA

By

H. MERSKEY, M.A., B.M., B.Ch., D.P.M.

Assistant Psychiatrist

Cherry Knowle Hospital, Nr. Sunderland

INTRODUCTION

BENEFICIAL results with procaine amide in the treatment of Huntington's Chorea were first reported by Goldman (1952). He was led to make an oral trial of the drug after having noticed a patient in the dentist's chair improve following an injection. He found improvement in six cases with the typical syndrome of Huntington's Chorea, of whom four had a positive family history of the disease. The evidence of improvement in these cases was partly based upon clinical observation and partly upon the patient's account of changes in his daily experience, for example, his first case had difficulty in eating, which had led to protests from other customers at a restaurant he had frequented, and this disability is said to have been satisfactorily reduced with the help of the drug.

DeMyer and Dyken (1954) studied nine cases of Huntington's Chorea using procaine amide one gramme four times a day, as employed by Goldman. They found no objective evidence of improvement but remarked that seven patients unequivocally maintained that they were better. Pleydell (1954) treated six cases with oral procaine amide and noted "no beneficial effect which could be attributed to the use of the drug". Lazarte *et al.* (1955a) observed three patients of whom two were co-operative and all had a positive family history. They provided standardized tests of manual ability in addition to taking motion pictures and making clinical observations, and they found much day-to-day variation but no consistent improvement either subjectively, or on clinical observation or in the performance of the objective tests. Their scheme of dosage, however, departed from that of Goldman in significant respects. Most recently, Forrest (1957) using finger-dexterity tests in two cases and scoring the involuntary movements in two others, has been unable to confirm the occurrence of any reduction in hyperkinesia from the administration of procaine amide or any improvement in performance. He believed more benefit to follow from the use of reserpine, as did Lazarte *et al.* (1955b) and Chandler (1955).

Favourable reports on East Indians with the disease have been made by Cohen (1956) and Ganguli (1956). Cohen's case showed objective clinical benefit from as little as 0.25 g. procaine amide, b.d., and this improvement was consistently maintained and extended to the mental state of the patient who was better able to perform mental arithmetic. Ganguli's case showed subjective benefit and some improvement in movement.

Following on these mixed reports, this paper presents an attempt to evaluate the worth of the drug by using both clinical and formal objective tests in an adequate number of patients.

MATERIAL AND METHODS

Six male patients and two female patients, all with mental changes beginning in adult life, were investigated. In five of these cases there is a history of closely similar illness in a parent or sibling. In one more case (J.J.F.) the patient's testimony is inaccurate and unsupported by other evidence but he states that his sister is also affected. Both parents, however, reached the age of sixty-eight and died without showing definite overt signs of the disease when visited in their home by the psychiatric social worker. In a further case (L.M.) the patient's antecedents are unknown and difficult to trace and there are indications that her parents may have been psychologically abnormal. Lastly in the remaining case without a family history of the illness (G.W.G.) the patient's father died at the age of forty-nine in a way which could conceivably be due to the commencement of choreic disturbances, having a fatal fall while working at his trade of steeple-jack.

The average age of the patients at the time of study was fifty-four years (to the nearest year), the youngest being forty-three and the oldest sixty-six, and the average known duration of the illness from the date of observed onset was eight years. In no case was there reason to suspect other types of organic illness in the central nervous system and in five cases the constituents of the cerebrospinal fluid, the Wassermann reaction and Lange curves in the cerebrospinal fluid, and the blood Wassermann and Kahn reactions were known to be normal. In the remaining three cases where only the Wassermann and Kahn reactions in the blood were known there was also no serological abnormality.

Observations were made to the same pattern in all cases except L.M., whose clinical course is described separately. The observations were made first before treatment with procaine amide hydrochloride (Pronestyl, Squibb). The remaining seven patients were then given procaine amide orally in increasing doses reaching the level of 1.0 g., q.i.d. in all but one case, J.McA., who became drowsy when receiving 0.5 g., q.i.d. and was then given d-amphetamine 5 mg. morning and noon together with this lesser dose of Pronestyl. The individual top dosage was then maintained in these seven patients for four weeks and the preliminary observations were repeated during the fourth week. The procaine amide was then stopped in the same seven and the final observations were made in the third week after complete cessation of the drug.

Five types of observation were taken into account throughout:

1. Subjective report.
2. Details of nursing needs, physical activity and habits.
3. Examination of the involuntary movements and physical state.
4. Written records of performance.
5. A block-sorting test of manual dexterity.

Special observations were made on the mental state before and during treatment but were subsequently abandoned as showing no difference of note between these two periods.

The observations on the physical state consisted of: (a) noting the presence and range and counting the frequency by stop-watch of the involuntary movements; (b) noting the performance in the finger-nose co-ordination test; (c) examining the gait; and (d) making any additional observations which were particularly relevant (e.g. whether there were any changes in the Parkinsonian features in the case of V.F.).

In securing written records the patient was presented with a pencil and a duplicated form on which he was requested to draw a straight line between two marked points and to write—"I see the cat" on a marked line. Each of these actions was requested three times on each occasion before, during and after treatment and the written instructions were supplemented by word of mouth.

The block-sorting test was deliberately chosen for simplicity and ease of comprehension by demented patients. Even so it was beyond the capacity of one case (L.M.) and almost beyond that of another (J.McA.) who required repeated careful instructions. It consisted of a tray with a central grey compartment $10 \times 7.5 \times 1.4$ inches and one white and one red compartment on either side, each of the latter being $7 \times 7.5 \times 1.4$ inches. All the compartments were open at the front. Twenty red and twenty white blocks $1.4 \times 1.4 \times 0.8$ inches were mixed together in the central grey compartment and the subject was instructed to sort them out into the compartments of their own colour, red in red and white in white, the process being first demonstrated and the patients being instructed to carry it out as quickly as possible without regard to tidy placing of the pieces so long as they were dropped and remained in the appropriate compartment. The particular piece of apparatus used was specially constructed for this trial and made from wood. Average normal adult subjects complete the sorting in from sixteen to thirty seconds as a rule and after repeating their initial run-through show no appreciable gain in speed due to learning. Mentally defective patients who were chosen to provide some near standard of comparison with the Huntingtonian cases took from fifty to seventy seconds. They were mainly high-grade epileptic imbeciles and showed a tendency to cross their hands in sorting and make uneconomical and clumsy movements. They also showed no significant improvement due to learning after the initial explanation and run-through. Like them the patients in the series were given the opportunity of familiarizing themselves with the test before the first series of observations was begun.

RESULTS

A. Case Histories

Case 1: J.J.F., male, 53 years old. Married. Separated. Parents said to be cousins but both reached old age without showing definite signs of choreic or psychological illness. One sister, not seen but said by patient to have same condition. Illness manifest in 1949 with sudden outbursts of rage and "dirty habits". But he had previously been discharged from the Army on psychiatric grounds as unable to drill and a persistent absentee. In 1951 admitted to hospital in a state of dementia and in 1955 choreiform movements were clearly apparent. Cerebrospinal fluid normal and blood Wassermann and Kahn reactions normal. At the time of this investigation he was slovenly in his person and tended to be doubly incontinent several times a week. His speech was explosive, slurred, incoherent and fragmented; gait unsteady and lurching and he was unable to walk heel-to-toe; subject at rest to more than forty involuntary movements per minute in each upper limb and could hardly touch his nose with his finger when requested. The dementia is evidenced by his idea of the date—24 August, 1939—the actual date being then April, 1957, and by his answer of thirty-four to the request to take seven from one hundred.

During treatment with procaine amide the performance of the finger-nose test improved and his finger-placing was relatively accurate and his incontinence was less (about once a week instead of thrice) but there was no improvement in gait or orientation. After stopping the procaine amide the slight improvement in his habits remained but performance in the finger-nose test deteriorated to its former level. Subjectively he was pleased with the tablets while having them but once they were stopped he was got to say that he was better without them.

Comment: No definite evidence of improvement.

Case 2: V.F., female, 43 years old. Married. Her grandfather died in a mental hospital, her father died of unknown causes, a brother committed suicide and a sister died in Cherry Knowle Hospital with chorea and dementia. Signs of illness appeared in 1947 with a facial

tic and mental changes when she was admitted to Cherry Knowle Hospital during her fifth puerperium. In 1957 immediately before treatment with procaine amide she was dementing steadily and was fatuous, facile, unoccupied and disorientated. Physically she was plump and slow in voluntary movement. Her speech was slurred and limited to single words and she showed one or two irregular movements in one part or another successively, the rate in each arm being about one to two per minute. The pupillary reactions and fundi were normal but the facial musculature was mask-like and spastic despite an occasional grimace. The only other abnormality in the cranial nerves was a bilateral partial nerve-deafness. Her hands were hyperhidrotic yet cold and her arms showed obvious cog-wheel rigidity with pathologically increased tendon-reflexes; performance in the finger-nose test was mildly impaired and her voluntary finger movements were stiff. In the legs there was a lead-pipe mixed spastic and Parkinsonian rigidity and rapid rhythmical sustained fine clonus obtainable at the left patella and both ankles; the tendon-reflexes were much increased but the plantar responses were flexor. Her gait was spastic on a wide base and she was thought to have the widespread changes of the disease referred to by Bell (1934) and which are not infrequent in advanced cases.

Under treatment with procaine amide the mental state and physical signs described altered only in the following respects. Involuntary movements were about five to ten per minute in each upper limb, her gait was stiff but improved and her performance in the finger-nose test was accurate. However, she said the tablets were "awful" and made her feel sick and during the period of treatment with procaine amide she developed an infection of her left fourth finger for which her wedding-ring had to be removed; there was no agranulocytosis however or systemic disturbance and the infection responded satisfactorily to penicillin. After stopping the procaine amide her speech and gait appeared worse and her feeding habits more slovenly. Her conversation was reduced to slurred syllables only. Her performance in the finger-nose test however remained fairly accurate and there was no other change of note. Involuntary movements were at a frequency of five to seven per minute.

Comment: No definite evidence of improvement.

Case 3: G.W.G., male, 58 years old. Married. Separated. His father died aged 49 from a fall whilst working as a steeple-jack. No other family history available to suggest hereditary chorea. Illness manifest in 1955 when he ceased working as an electrician's mate in a transport undertaking. Increasingly worse after admission to hospital and subject to an organic type of dementia with paranoid delusions additionally; thus he was poorly orientated and when asked to take seven from one hundred answered "eighty-nine" and he also asserted that he was in the C.I.D. and in charge of the hospital. Before treatment with procaine amide his speech was rambling, slurred and incoherent. Involuntary movements were present at a rate of some thirty to forty per minute in each upper limb. Performance in the finger-nose test unsteady and inaccurate; gait unsteady and shuffling and unable to walk heel-to-toe. Wassermann and Kahn reactions normal in blood and cerebrospinal fluid.

During treatment with procaine amide involuntary movements were reduced to less than ten per minute in each upper limb; no other signs of improvement or deterioration however. Condition essentially the same on stopping treatment but involuntary movements returned to thirty to forty per minute in each upper limb. Subjectively unchanged maintaining throughout—"There's nothing wrong with me".

Comment: The hyperkinesia did appear to benefit. Otherwise no improvement occurred.

Case 4: G.A.H., male, 43 years old. Married. Mother died in Cherry Knowle Hospital with chorea and dementia. Patient had been deaf for many years (secondary to old otitis media) and prone to temper tantrums all his life. Admitted to hospital in 1954 with a pyrexial illness finally labelled influenza but accompanied by pain in the head, back and legs. His cerebrospinal fluid was normal in all respects. In 1957 he gave a history of generalized pains in the arms and legs, present for ten years, and in January, 1957 he complained of pain in the lower part of the back, radiating to the right leg and ankle and sometimes to the left. At that time the physician who examined him observed that there was a scoliosis and limitation of straight leg raising to 45° on the right side but no muscle spasm and no other abnormality on spinal radiography. It was thought further that the scoliosis was grossly exaggerated by the patient and that the limitation of his spinal movements which was observed was voluntary. He then came under psychiatric care and was observed to show some slurring of speech and choreic movements at rest which were exacerbated during voluntary activity; there was also a typical choreic disorder of muscle control evident in the grasp and worse with excitement or during attempts to co-operate by making particular voluntary movements. His legs were frequently drawn up while he grimaced with pain and his hamstring muscles went into fluctuating spasm and he was tender over his sciatic spines and also over the right ilium in the region of the hip-joint. There was no other abnormality in his motor and sensory systems on objective examination. His emotional state was one of euphoria and mild fatuity except in relation to his pains where his attitude was passably normal.

Further investigations revealed no local cause for these pains although they were thought at first by a consultant physician to be due to local irritation of peripheral nerves by carcinomatous deposits. Radiographs of the pelvis, hips, femora, humeri and both shoulders showed no abnormality; plasma proteins, serum calcium and phosphorus and acid and alkaline phosphatase estimations were within normal limits and there was no protein (including

Bence-Jones protein) in the urine. On 14 March, 1957, his haemoglobin was 16.6 g. per cent., white blood cells 16,500/c.mm., neutrophils 76 per cent., lymphocytes 16 per cent., monocytes 6 per cent., eosinophils 2 per cent. and the E.S.R. 22 mm. at 1 hour (Westergren) and there was some nasal catarrh but no pyrexia. By 27 March, 1957 these figures had altered to W.B.C. 8,700/c.mm., with a normal differential count and the E.S.R. was 4 mm. at one hour. These were the only abnormal laboratory findings at any time and meanwhile the administration of procaine amide was commenced beginning with 0.5 g. q.i.d. on 9 March, 1957 and rising to 1.0 g. q.i.d. on 18 March, 1957 and subsequently for four weeks as with the other cases. The pains did not respond to aspirin or other simple analgesics or even to pethidine 50 mg. by mouth and they continued to cause him immense concern. They were finally regarded as either purely functional or as an unusual manifestation of the central neuropathic process of Huntington's Chorea. Although this has not been previously recorded the latter conclusion is not untenable in the light of subsequent developments.

Before receiving procaine amide, the patient was complaining of numerous pains in the leg and he lay about with his limbs drawn up, quite unoccupied. Involuntary movements were at a frequency of three to four per upper limb per minute and his performance in the finger-nose test was excellent. During treatment with procaine amide less than none to one involuntary movements per upper limb per minute were observable and his pains underwent a steady improvement and were abolished and he could perform straight leg raising to ninety degrees and walk about easily. On ceasing procaine amide pain soon re-appeared in his back although not to the same degree as formerly and he could still walk about although unsteadily. The involuntary movements were at a frequency of 15 per upper limb per minute.

At the end of the trial period he was re-started on procaine amide 1.0 g. q.i.d., and discharged from hospital symptom-free. This state continued for one month when the dose of his procaine amide was reduced to 0.25 g. q.i.d. and he soon re-appeared at the out-patient department with his old pains and hobbling. Since restoring the dose to 1.0 g. q.i.d., he has again improved.

Comment: There was considerable objective and subjective benefit in this case but the exact symptoms that improved most are not ordinarily linked with Huntington's Chorea. The more typical Huntingtonian features showed slight improvement.

Case 5: J.McA., male, 53 years old. Widower. His father died in another mental hospital with some of the physical signs of general paresis at the age of 38 years. No serological evidence was recorded. His sister, C.E., died in Cherry Knowle Hospital with gross chorea and dementia; she had previously contracted G.P.I. which was treated with full doses of penicillin and with malarial therapy but her deterioration continued after this treatment in 1948-49 until her death in 1952, aged 44 years.

The patient himself had no abnormality in the C.S.F. cells, protein, serological reactions and colloidal gold test and his blood Wassermann reaction and Kahn test were negative. He was a joiner and at first was a good workman and husband. In 1939 he fell ill for the first time after joining the Home Guard from which he was speedily released. He had a "nervous breakdown" and was off work for eighteen weeks and his wife regarded him as altered from that time, being irritable and unruly. In 1951 he was admitted to hospital for the first time after having attacked his wife with violence and choreic movements were first recorded in 1955. The movements were very variable in their frequency at different times both before, during and after treatment, ranging from none to forty per minute for all parts of the body taken together so that no effective comparison was possible.

Before treatment his speech was restricted in content and the patient was grossly disorientated; speech slow and intelligible, occasionally explosive but never more than a few syllables uttered at a time. Performance in finger-nose test accurate and gait steady. No evidence of improvement during treatment but subjectively pleased with tablets, saying—"They give you strength".

Comment: No evidence of significant improvement or deterioration.

Case 6: L.M., female, 66 years old. Married. Separated. Family history obscure and indefinite as patient's own father and siblings were unknown to her children, patient's mother died of asthma aged 42 years, and patient was reared by a step-father. Signs of mental illness are said to have appeared in 1940. In 1944 she was treated temporarily in hospital for "nerves and jaundice". In 1950 she was admitted to a London mental hospital suffering from a "paraphrenic psychosis" and in 1952 she was transferred to Cherry Knowle Hospital, at which time choreiform movements were evident.

In this case it was not possible to follow the same plan of investigation as in the others; the reasons for this will be apparent from the account following.

In January, 1957 she was up and about but was very thin and subject to incessant involuntary movements in all parts at a rate well exceeding eighty per minute in each of her upper limbs. She was fatuous and demented, her speech was very limited and slurred and she was extremely greedy, bolting any food within her reach. She was then given procaine amide in doses rising to 0.75 g. b.d. with the apparent effect of slightly lessening her movements. After four weeks, however, she appeared to be still deteriorating generally and the drug was stopped on 24 February, 1957. Within twenty-four hours of stopping the procaine amide however the illness took a dramatic and unexpected turn. Her movements increased in number and

amplitude quite markedly beyond anything previously seen and they were so extensive and she was so agitated that she could hardly be kept within her cot-bed. In fact on the same day she fell over the side and sustained very extensive bruising. For the next two days she was nursed in the protection room and sedated with barbiturates and on 28 February, 1957 procaine amide was recommenced in increasing doses. Before resuming the drug she was fatuous, euphoric and dirty in her habits. She handled faeces indifferently on the floor of the protection room before wiping her face and she did not have one syllable of coherent intelligible speech. Her movements were as described. On 5 March, 1957 after three days' treatment with procaine amide 0.5 g. 6-hourly she had improved and could again be nursed in the open ward. She spoke enough to say—"I'm doing fine now" and to give her Christian name and her movements were less extensive although not apparently diminished in frequency; and she could walk unaided. At this time she looked toxic and this was attributed to the effects of absorption of extravasated blood. Further increases in the dose of procaine amide were made but had no beneficial effect that could be detected and by 12 March, 1957 the movements were again becoming extensive and troublesome; even her tongue protruded irregularly at an uncountable rate. Difficulty in swallowing appeared and the same day she could only take fluids and her 6.00 p.m. dose of procaine amide was only taken in part; steady deterioration continued until she achieved a restless sleep with the help of amylobarbitone sodium, grains iii. Next morning, 14 March, 1957 at 2.30 a.m. she vomited about a pint and a half of brownish fluid and died half an hour later, pale and exhausted.

At autopsy no particular immediate cause of death could be demonstrated and it was supposed that her end was directly due to the Huntington's Chorea, occurring in a fashion observed in others with this illness.

Comment: Symptomatic benefit of moderate degree was considered to have resulted from the use of procaine amide in this case, but without influencing the final outcome or progress thereto. The events immediately after the stopping of the drug raised the question of a "rebound phenomenon" which is further discussed below.

Case 7: N.M., male, 62 years old. Single. Two brothers stated by acquaintance of family to have died of illness similar to Huntington's Chorea and one other sibling said to be affected. Illness known to have been present since 1950 but not admitted by patient who was certified on his admission to hospital in 1956. He had been in receipt of a disability pension for nearly total deafness due to wounds suffered in the 1914-18 War and had not worked for many years. He was cared for by his landlady who finally could manage him no longer and at the time of his admission to hospital showed slurring of speech and generalized choreo-athetoid movements. There was no abnormality in the cerebrospinal fluid and the blood Wassermann reaction was normal. Before receiving procaine amide he was subject to about ten involuntary movements a minute in each upper limb; his speech was repetitive, incoherent and slurred and his performance in the finger-nose test was accurate but jerky. His gait was lurching and he was altogether very much worse when attempting set tasks or when excited.

Under treatment with procaine amide he was subjectively no better, saying—"There's nothing wrong with me. I don't need medicine." Movements were at a frequency of twenty to thirty per minute in each upper limb and there was no definite improvement (or deterioration) in speech, co-ordination or gait. On ceasing procaine amide his movements were at a rate of sixty per minute in each upper limb and appeared somewhat increased in amplitude and his performance in the finger-nose test was inaccurate as well as jerky. He protested how well he was and his gait and speech were unchanged.

Comment: There was no indication of definite improvement but this case (like Case 6—L.M.) also raised the question of whether deterioration had occurred since the start of treatment or whether there was a "rebound phenomenon".

Case 8: J.W.W., male, 57 years old. Divorced. Cause of death of his parents unknown but his daughter was admitted to a hospital in the London region in 1956 with signs of a "rapidly progressive form of Huntington's Chorea". Other members of family of J.W.W. were also believed to have been affected. The patient was known as a conspicuous figure with choreiform movements even before his admission to hospital in 1953 on the death of his father.

Immediately before the administration of procaine amide he was being nursed in a cot-bed. He was deluded that the nursing staff were grossly ill-treating him and he was disorientated and frequently used very foul language. His speech was slurred and incoherent and he was subject to numerous grimaces, head movements and movements of the limbs at a rate of more than eighty per minute in each part. His tongue sometimes protruded and the head and limb movements were of considerable amplitude; following any excitement or an enema they were worse; at meal-times he broke crockery some two or three times a week owing to his lack of control and when he had a bath it was the usual practice to sedate him with amylobarbitone sodium grains iii, as otherwise there was no possibility of his escaping harm owing to the amplitude and vigour of his involuntary movements. He could scarcely do formal tests of co-ordination and his performance in the finger-nose test was wildly inaccurate.

During treatment with procaine amide he was observed to be much less querulous and complaining (without being drowsy). His movements were still more than eighty per minute

in each upper limb but their amplitude was thought to have diminished. There was some improvement in his performance of finger-nose test and his performance in the written tests was distinctly better (he was the only case in which this was so). He spilt less food and rarely broke plates and although he looked thinner and more "peaky" his weight in fact fell by less than three pounds from an original one hundred pounds. During this period of treatment with procaine amide he was also receiving a prophylactic course of sulphaguanidine owing to the occurrence of dysentery in the ward.

On stopping the procaine amide the movements of each part became, if anything, more frequent (although they were already so numerous as to make comparison difficult). They were of greater amplitude however and again necessitated sedation before his bath and it was also necessary to give him a tin basin from which to feed in place of crockery. Further, immediately after stopping the procaine amide his tongue made frequent involuntary protrusions, keeping his lips wet, a phenomenon not previously observed and which disappeared in two to three days. His subjective response was favourable whether he was receiving tablets or not.

Comment: It was considered that some improvement occurred under the drug and this was another case in which the occurrence of a "rebound phenomenon" appeared quite likely.

On these clinical assessments three patients (G.A.H., L.M., and J.W.W.) are regarded as having shown some acceptable improvement from the administration of procaine amide but in one of them the symptoms which seemed most particularly to be benefited are not characteristic of Huntington's Chorea. In these cases the general improvement is partly parallel with some reduction in the frequency of involuntary movements. The figures obtained for the latter are summarized in Table I and it will be observed that two other patients (G.W.G., and J.McA.) also showed this trend but that one patient (V.F.) showed most movements during the period of treatment (which she disliked). The remaining two patients (J.J.F. and N.M.) showed more movements during treatment than previously but more still on ceasing treatment.

TABLE I
Frequency of Involuntary Movements at Rest

Patient	Movements in Each Upper Limb per Minute		
	Before Procaine Amide	During Treatment with Procaine Amide	After Procaine Amide
J.J.F.	40-50	60-80	Over 80
V.F.	1-2	5-10	5-7
G.W.G.	30-40	10	30-40
G.A.H.	3-4	0-1	15
J.McA.	2-3	0-2	5-7
	(Very variable)		
L.M.	Uncountable	Uncountable	Uncountable
N.M.	10	20-30	60
J.W.W.	Over 80	60-80	Over 80

B. Formal Tests

The written tests before, during and after treatment were given to all cases except L.M. In one instance (J.W.W.) they showed a marked improvement during the treatment period but much of this improvement, although not all, was maintained after treatment. The other cases showed no significant variation.

The results for the block-sorting test are set out in Table II. In four out of seven cases the best mean performance time occurred in the treatment period. In two cases (J.J.F. and J.McA.) the mean times worsen steadily on each occasion and in one case (G.A.H.) they improve steadily on each successive occasion. The overall mean time taken before treatment was 1 minute 21 seconds, during treatment 1 minute 10 seconds and after treatment 1 minute 32 seconds (to the nearest second). These results show a surface trend favouring the treat-

ment. However, the standard deviation of only one of them (the mean figure during treatment of 1 minute 10 seconds) is 40 seconds and without further calculation it is apparent that these figures are not statistically significant.

TABLE II
Performance Times in Block-Sorting Test of Manual Dexterity

Patient	Before				During Treatment with Procaine Amide				After			
	Procaine Amide				Procaine Amide				Procaine Amide			
	Test Times				Test Times				Test Times			
J.J.F.	133	151	127	137	144	146	158	149	168	142	144	156
V.F.	97	90	97	95	85	72	71	76	124	124	136	128
G.W.G.	65	37	46	49	53	46	37	45	59	46	40	48
G.A.H.	45	38	41	41	37	39	33	36	30	36	34	33
J.McA.	90	97	98	95	111	108	90	103	119	108	123	117
N.M.	62	55	55	61	27	27	27	27	34	29	33	32
J.W.W.	99	83	90	91	49	55	57	54	68	76	74	73
Average of mean scores	..	..	..	81	..	..	..	70	..	..	..	83

The times taken are given in seconds to the nearest second.

Standard deviation of average of mean scores during treatment = 40 seconds.

SIDE EFFECTS

Procaine amide is widely used in the treatment of cardiac arrhythmias and a considerable literature exists on its administration and possible toxic properties. In general it appears to be a fairly safe drug, its commonest side effects being nausea, vomiting and gastric discomfort. In the one case in this series (V.F.) where vomiting occurred, it ceased on the withdrawal of the drug. The same patient, as recorded, had a finger infection while receiving the drug but showed a normal leucocyte response. Agranulocytosis has, however, been recorded (Inouye *et al.*, 1951; Miller *et al.*, 1951) and is an occasional hazard as are severe hypotensive reactions and ventricular fibrillation. These latter phenomena do not appear to occur with the oral form of the drug.

In this series there were no untoward hypotensive reactions and the blood-pressure did not vary excessively in the cases in which a worthwhile reading was obtainable (i.e. all except L.M. and J.W.W.). Likewise there was no granulocytopenia found on routine counts of the white blood cells in all cases during the treatment period and the only additional occurrence to remark is the somnolence which appeared in J.McA.

DISCUSSION

Textbook accounts of Huntington's Chorea (Brain, 1955; Kinnier Wilson, 1940) tend to stress the inexorable, unremitting course of the disease; so also does the major account of the illness given by Bell (1934). Until the studies of Lazarte *et al.* (1955a) no one seems to have thought it worth while to comment on the considerable day-to-day fluctuation which can occur in the frequency and amplitude of the involuntary movements. It is however, well recognized that sleep abolishes the movements in various forms and in hemiballismus and it should not be surprising conversely if excitement exacerbates them. In fact Wilson (1940) did recognize these variations in Sydenham's Chorea, and such changes have often been observed in the patients in this series. Further, although in this study care was taken to count the movements under quiet conditions when the patient was not excited or too much interested in his surroundings it proved very difficult to obtain conditions that could be regarded with satisfaction as "basal". Indeed so long as the immediate presence of the doctor is noticed by the patient it is impossible to assume that such conditions are present

and accordingly only limited weight can be placed upon observations that the movements are decreased unless this effect is consistently maintained. Likewise, the character of the subjective response to the drug is seen to be of little help in determining its value and can only be assessed by a personal appreciation of the patient's attitudes, as will be evident from the reports of several of these cases, particularly V.F. and L.M.

The results of formal test have been inconclusive in other studies (Lazarte *et al.*, 1955a; Forrest, 1957) as in this one. When the wide range of facility in these patients is considered and the many factors capable of affecting the results (including the mental state of the patients) this is not perhaps surprising. It still may be of more value in assessing the worth of procaine amide in this condition if clear examples can be given of facultative improvements with or without the drug. Goldman (1952) does this for procaine amide as do Lazarte *et al.* (1955b) for reserpine, and in the present series where improvement has been accepted as taking place it has been accepted in substantial part on that basis.

The possible occurrence of a "rebound phenomenon" on cessation of the procaine amide was first remarked in the case of L.M. and again in that of J.W.W. In the later cases it seemed of less significance but the case of L.M. discouraged the use of a blind trial of the drug using a placebo. In the light of the other cases this is now a less serious consideration. From the fact that in J.W.W. some of the "rebound phenomena" improved over the course of a week it would appear unlikely that the apparent "rebound" was due to the unmasking of deterioration occurring during the treatment period and concealed by the taking of the drug. And also from the limited evidence so far available it does not seem as if the occurrence of this "rebound phenomenon" need be a contra-indication to giving the drug to patients who might benefit, since it subsides quickly and the patient can be tided over the intervening period with barbiturate sedation if necessary. But like chlorpromazine in excited patients procaine amide in Huntington's Chorea may have the advantage over barbiturates of producing symptomatic benefit without causing undue somnolence.

In this series the majority of patients did not benefit from the drug but some did show what was regarded as valid improvement and it still seems to be the correct policy to give it an adequate trial in all cases of adult chorea unless contra-indicated.

SUMMARY

Procaine amide hydrochloride (Pronestyl, Squibb) was given to eight patients with Huntington's Chorea. Seven of these patients were subjected to tests of manual dexterity and accuracy and all the patients were assessed clinically before, during and after treatment with the drug. There was no significant improvement in test performance during the period of drug treatment but two cases showed symptomatic motor benefit. Of these two, one died, it is believed, from the ordinary progress of the disease. Six patients showed no definite signs of improvement or deterioration. Attention is drawn to the difficulty of assessing alterations in the frequency of the involuntary movements and the occurrence of a possible "rebound phenomenon" on ceasing the drug is noted. It is concluded that procaine amide should be given an adequate trial in all cases of adult chorea unless contra-indicated.

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REFERENCES

- BELL, J., *Huntington's Chorea in Treasury of Human Inheritance*, 1934, Vol. 4, pt. 1.
BRAIN, W. R., *Diseases of the Nervous System*, 1955. London: Geoffrey Cumberledge, Oxford Medical Publications.
CHANDLER, J. H., *University of Michigan Med. Bull.*, 1955, **21**, 95.
COHEN, C., *East African Med. J.*, 1956, **33**, 104.
DEMYER, W., and DYKEN, M., *Amer. J. Med. Sci.*, 1954, **228**, 70.
FORREST, A. D., *J. Ment. Sci.*, 1957, **103**, 507.
GANGULI, L. K., *J. Indian Med. Ass.*, 1956, **26**, 274.
GOLDMAN, D., *Amer. J. Med. Sci.*, 1952, **224**, 573.
INOUE, M., MILLAR, J., and TOWNSEND, J. H., *J. Amer. Med. Ass.*, 1951, **147**, 652.
LAZARTE, J. A., BAARS, C. W., and PEARSON, J. S. *Amer. J. Med. Sci.*, 1955a, **229**, 676.
Idem, *Proc. Mayo Clin.*, 1955b, **30**, 358.
MILLER, H., POLLOCK, R. G., and GRIFFITH, G. C., *J. Lab. and Clin. Med.*, 1951, **38**, 850.
PLEYDELL, M. J., *Brit. Med. J.*, 1954, *ii*, 1121.
WILSON, S. A. K., *Neurology*. 1940. London: Edward Arnold & Co.

MELANCHOLIA, GLUCOSE TOLERANCE AND BODY WEIGHT

By

I. G. PRYCE, B.Sc., M.B., B.S., D.P.M.

Research Registrar

Runwell Hospital, near Wickford, Essex

A DECREASE of glucose tolerance in mental illness has been observed for over forty years (Kooy, 1919; Mann, 1925). Although this change in carbohydrate metabolism has been described in all types of mental disorders, the majority of investigators report that it occurs most frequently in melancholia (Kooy, 1919; Mann, 1925; McCowan and Quastel, 1931; Holmgren and Wohlfahrt, 1944). The phenomenon has usually been regarded as a secondary effect, and several theories have been suggested to explain how it is produced (Kooy, 1919; Mann, 1925; McCowan and Quastel, 1931; Holmgren and Wohlfahrt, 1944); however, no single explanation is firmly established, and the mechanism or mechanisms are still uncertain.

The present investigation was undertaken to re-examine glucose tolerance in depressions. To obviate the possible effect of variations in the intestinal absorption of glucose, a factor previously held to be of paramount importance in determining the shape of glucose tolerance curves in depressions (Greville, 1945), intravenous are preferable to oral tests. Moreover, since the absorption factor is eliminated, the curve obtained by the intravenous test reflects the rate of glucose utilization alone, and this curve corresponds closely to an exponential equation (Greville, 1943). Thus, by means of a suitable technique and appropriate mathematical treatment, it is now possible to express glucose tolerance as a single numerical index (Duncan, 1956); this facilitates statistical analysis in studies of glucose tolerance.

A control group of mentally healthy subjects of similar age distribution to that of the depressions was examined concurrently. Systematic observations of various physical and mental factors were made in both experimental and control groups in the hope of establishing correlations should any change in glucose tolerance be found.

METHOD AND MATERIALS

A slight modification of the method of Duncan (1956) was used in the present investigation. The subject was fasted for 15 hours (overnight) and remained in bed on the morning of the test. Three samples of 0.1 ml. capillary blood were taken from a stab in the lobe of the ear, over a period of 10–15 minutes. Fifty ml. of 50 per cent. dextrose were then injected into an arm vein within 3 minutes, the time exactly 4 minutes from the end of the injection was taken as zero, and the first post-injection blood sample obtained. Further samples of 0.1 ml. capillary blood were obtained at approximately 7 minute intervals over the next hour, alternative samples usually being duplicated. The time of collection of each sample was accurately recorded, the time half-way between beginning and end of collection being taken when collection took longer than 30 seconds. Dabbing the stab wound with cotton wool soaked in

heparin prevented blood clotting. Since a fairly large needle (Record fitting "VIM" 18 gauge) was needed for the injection of the syrupy dextrose solution, its site of introduction into the skin of the arm was anaesthetized with a small intra-dermal wheal of 2 per cent. procaine. The glucose content of the samples was estimated by the micro-method of Folin and Wu, with the aid of a null-point photoelectric colorimeter. By plotting the logarithm of the difference between the mean fasting blood glucose and post-injection blood glucose values ($\log_2$ glucose-increment) against time from zero, a linear relationship is obtained. The slope of this line, as given by $k = \log_2 2/t_r$, where t_r is the time taken for the blood glucose increment at any point to be halved, is an index of the rate of glucose utilization (Duncan, 1956).

The experimental group (Table I) consisted of 19 patients admitted to a mental hospital because of depression. These patients were mainly consecutive admissions and were unselected. Those who gave such scattered blood glucose values that an unequivocal linear relationship between time and $\log_2$ glucose increment could not be obtained, were rejected from the group; some of these were early cases, and the scatter was possibly due to errors of technique. The group included 17 women and 2 men, ranging from 45 to 72 years of age, with mean age, weight and height for the women, of 57.4 years, 54.6 kgm., and 159 cm. respectively. Fifteen were recent admissions. Eleven gave histories of attacks of severe depression in the past, ten had a family history of mental illness or suicide, and sixteen either a past personal history or a family history of mental illness. In seven the depression was classified as an episode of manic-depressive psychosis, in eight as involutional melancholia, one was regarded as reactive (but she had a strong family history of depression), one was labelled neurotic depression, and two were unclassified depressions. Fifteen patients were treated with E.C.T.; fourteen gave a good response, in one the response was fair, and in four the improvement obtained was temporary. Recent admissions were tested during their first week in hospital, before commencing E.C.T.; all patients were depressed at the time of testing. Each patient's symptoms and signs were recorded on a check-list; and systematic observations of behaviour during testing were made on fourteen patients.

The control group (Table II) consisted of nine women, aged 42 to 71 years, with a mean age, weight and height of 61.1 years, 67.6 kgm., and 159 cm. respectively. Four had had surgical operations 10 days to 3 months previously, two had long-standing hemiplegias and one multiple arthritis.

Information was sought from subjects and relatives in both groups concerning past history and family history of illness; weight over the year and diet and appetite during the three to six months before admission; type of onset and length of illness; and (from nursing staff) amount of diet during the four days before the glucose tolerance test was carried out.

RESULTS

Table III shows that there is a highly significant decrease in glucose tolerance in the group of depressions compared with the control group. Duncan (1956) gives an "increment index" of 3.68 (range 3.15-4.62; s.d. 0.4) for healthy adults between 20 and 60 years; and a value of 1.83 (range 1.33-2.34; s.d. 0.31) for mild diabetics. The mean value found in depressions thus lies near the upper limit of that given for mild diabetes.

The mean weight of the depressions is 13 kgm. (2 stones) less than the mean weight of the controls; this difference is highly significant (Table IV). The mean

TABLE I

No.	Sex	Age	Weight (kgm.)	Height (cm.)	Past History	Family History	Weight Before Admission	Appetite Before Admission	Pre-test Diet	Length of Illness	Onset	Response to E.C.T.	"Inclement Index"
1	F	51	61.7	163	Dep. 26, 3, 2 years ago	Nil	Constant	Normal	Decreased	2 weeks	Acute	Good	1.87
2	F	47	55.3	165	Dep. 13 years ago	S. suicide, B. in M.H.	Constant	Normal	Normal	8 months	Subacute	Good	2.39
3	F	47	71.7	155	Nil	M. obese	?Decrease	Normal	Normal	6-7 weeks	?Acute	Good	2.27
4	F	72	40.4	157	Dep. 8 and 6 years ago	Nil	Decrease	Poor	Decreased	3 months	Subacute	Good	1.98
5	F	61	52.6	160	Dep. 3 and 1 year ago	M. and sisters "nervous"	Decrease	Fair	Normal	6 weeks	Subacute	Good	2.16
6	F	72	49.0	163	Chronic Dep. since 1937	S. to 10	Constant	(Fair)	Normal	19 years	Insidious	Good	2.77
7	F	56	53.1	157	Nil	Obesity	Decrease	Poor	Normal	6-8 months	Insidious	Good	2.10
8	M	47	63.0	152	Rec. dep. since 1947	S. suicide	Decrease	Fair	Decreased	4-6 weeks	Subacute	Good	1.44
9	F	65	48.5	183	Nil	S. to 6	Decrease	(Fair)	Normal	3 years	Subacute	—	2.04
10	F	62	45.8	163	"Breakdown" in 1924	S. to 12	Constant	Fair	Normal	16 weeks	Subacute	—	3.01
11	F	56	51.3	152	Rheum. arth. 10 years	S. to 11	Decrease	Poor	Normal	6 weeks	Acute	Good	2.16
12	F	66	57.2	155	Ost. arth. Rec. dep. 1 year	GF, M. and Aunt were in M.H.	Increase	(Normal)	Not observed	1 year	Subacute	Fair	2.20
13	F	63	49.4	157	"Breakdown" in 1929	Nil	Decrease	Poor	Normal	3½ years	Subacute	Good	2.99
14	F	53	43.1	157	Nil	GF, M. and Aunt were in M.H.	Increase	(Normal)	Normal	1 year	Insidious	—	3.46
15	M	45	54.4	165	Rec. dep. since 1936	Aunt in M.H.	Decrease	Fair	Normal	5-6 weeks	Subacute	Good	2.66
16	F	48	56.2	163	Rec. dep. since 1932	Nil known	Constant	Fair	Normal	1 week	Acute	Good	2.17
17	F	61	60.8	150	Rec. dep. since 1935	B. was in M.H.	Decrease	Poor (3 weeks)	Normal	3 weeks	Acute	Good	3.15
18	F	52	59.4	152	Rec. dep. since 1952	S. was in M.H.—Dep.	Constant	Normal	Decreased	4 months	Insidious	—	2.31
19	F	37	62.6	175	Dep. 17 and 3 years ago	Nil	Decrease	Poor	Normal	1 year	Subacute	Good	2.66

TABLE II

No.	Sex	Age	Weight (kgm.)	Height (cm.)	Past History	Family History	Weight Before Admission	Appetite Before Admission	Pre-test Diet	"Inclement Index"
20	F	42	68.5	155	Vaginal repair under G.A. 10 days previously	M. died of diabetes	Decrease (1st)	Normal	Normal	3.22
21	F	54	74.4	152	Multiple arthritis	Nil	Increase	Normal	Normal	2.77
22	F	67	67.6	165	Always "nervous"	S. has nervous breakdown	Decrease	Fair	Normal	3.33
23	F	65	74.4	163	"Nervous breakdown" years ago	Nil	Increase	—	Normal	3.22
24	F	63	72.1	165	Right hemiplegia 3 years	Nil	Constant	Normal	Normal	4.06
25	F	71	72.1	147	Right hemiparesis 6 years	Nil	Increase	Normal	Normal	2.31
26	F	54	70.8	155	Hysterectomy under G.A. 10 days previously	M. obese and diabetic	Increase	Normal	Decreased	3.65
27	F	70	50.8	165	Arthrodesis knee under G.A. 1 month previously	Nil	Constant	Normal	Normal	3.46
28	F	64	58.1	163	Osteotomy under G.A. 3 months previously	Nil	Decrease	Decrease	Normal	3.15

Abbreviations used in Tables I and II:
 Rec. dep. = Recurrent Depression. Rheum. arth. = Rheumatoid Arthritis. Ost. Arth. = Osteoarthritis. M. = Mother. S. = Sister. B. = Brother.
 Dep. = Depression. M.H. = Mental Hospital. G.A. = General Anaesthesia.
 GF. = Grandfather.

TABLE III

				Increment Index		
Depressions (females)	..		range 1.44-3.46;	mean 2.41;	s.d. 0.52	
Controls	..	..	range 2.31-4.06;	mean 3.24;	s.d. 0.50	
Difference	..	..	of means 0.83;	t=3.87;	D.F.=24;	p<0.001

TABLE IV

				Wt. in kgm.		
Depressions (females)	..		range 40.4-71.7;	mean 54.6;	s.d. 7.43	
Controls	..	..	range 50.8-74.4;	mean 67.6;	s.d. 8.04	
Difference	..	..	of means 13.0;	t=4.14;	D.F.=24;	p<0.001

weight of the controls, 67.6 kgm., approximates closely to 65.7 kgm., which is the mean weight of 24 normal subjects of similar mean age (63 years) given by Silverstone *et al.* (1957). No significant difference between the two groups is obtained in the history of change of weight before admission (Table V); and a history of decrease in appetite and diet during the three to six months before admission in the depressions compared with the controls is barely significant (Table VI).

TABLE V

Weight				Depressions	Controls	Total
Decreased	..	..	..	9	3	12
Constant or increased	..	..	..	8	6	14
Total	..	..	..	17	9	26

$$\chi^2=0.292; \text{D.F.}=1; p<0.7>0.5$$

TABLE VI

Appetite Before Admission				Depressions	Controls	Total
Fair or decreased	..	..	..	10	2	12
Normal or good	..	..	..	3	6	9
Total	..	..	..	13	8	21

$$\chi^2=3.538; \text{D.F.}=1; p<0.1>0.05$$

The diet during the four days before testing was reported to have been decreased in four of the seventeen female depressions, and in one of the nine controls; the difference between the two groups is not statistically significant ($\chi^2=2.38$; D.F.=1; $p<0.2>0.1$). The usual hospital diet offered adequate carbohydrate as was shown by weighing the daily intake of four patients (not included in the investigation) on four separate days; values of 306, 510, 231 and 306 gm. carbohydrate were obtained.

The relation between the glucose tolerance of the depressions and history of weight change before admission, diet and appetite before admission, and diet during the four days before testing, is as follows. The mean increment index of the ten who reported a decrease in weight is 2.33, and in the nine whose weight was reported to have been constant or increased the index is 2.49; the difference (0.16) is not statistically significant ($t=0.691$; D.F.=17; $p>0.1$). The mean indices for the eleven with decreased appetite, and the four with normal appetite before admission, are 2.34 and 2.31 respectively. The mean indices of the four with decreased food intake during the four days before

testing and the fourteen with normal intake before testing are 1.90 and 2.56 respectively; the difference (0.66) is highly significant ($t=11$; D.F.=16; $p<0.001$).

The mean increment index for the seven patients diagnosed manic-depressive psychosis (Cases 6, 8, 10, 14, 15, 16, 17) is 2.66, and that for the eight involuntional melancholias (Cases 4, 5, 7, 9, 11, 12, 13, 18) is 2.24. The difference (0.42) is highly significant ($t=5.872$; D.F.=13; $p<0.001$). Although the involuntional melancholics are slightly older and weigh slightly less than the manic-depressive group (mean ages 61 and 55 years; mean weights 50.2 and 53.2 kgm. respectively), the differences are not statistically significant ($K=0.369$ and 0.245 respectively; D.F.=13; $p>0.5$).

No correlation was found between various symptoms of melancholia and glucose tolerance. Thirteen patients were judged to have shown agitation, twelve retardation, and twelve anxiety or panics during the course of their illness. But there was considerable overlap, for eight patients had displayed both agitation and retardation at different times, while of the twelve anxious patients, ten were also agitated. The symptomatology of the two main groups of depression was essentially similar. Six involuntional and five manic-depressive melancholics were agitated and/or anxious; six involuntional and six-manic-depressive melancholics were retarded.

Of the fourteen depressions whose behaviour was recorded during testing, eight remained still and six were slightly resistive or agitated. The mean increment indices are 2.38 and 2.68 respectively. The mean pulse rate at the end of the test in twelve patients is 73/min. (range 60–84/min.). Of the nine controls, six were still, two rather restless, and one (Case 21) was in obvious pain from arthritis of the hip. Case 25 perspired freely throughout, and her pulse was 120/min. at the end; it was a fairly warm day. The mean pulse rate at the end of testing in five others is 78/min. (range 60–88/min.).

DISCUSSION

It has recently been shown that glucose tolerance decreases with age (Silverstone *et al.*, 1957). The two groups in the present investigation were however of similar mean age (57.4 and 61.1 years for depressions and controls respectively). Surgical procedures and arthritis are also known to be associated with decreased glucose tolerance (Epstein and Aschner, 1916; Fletcher, 1922), so that these complications in the controls tend to decrease rather than increase the difference in glucose tolerance observed between the two groups.

It is well known that deficiency of carbohydrate in the previous diet lowers sugar tolerance (see review by Chambers, 1938). The evidence obtained during this investigation suggests that the diet taken during the four days before testing supplied adequate carbohydrate in most cases, although more detailed knowledge of dietary intake would be needed to establish this conclusively. Other investigators have reported that their subjects took a diet adequate in carbohydrate content during the days before testing (Freeman *et al.*, 1944; Henneman *et al.*, 1954). The depressions, with a mean weight 13 kgm. less than the mean weight of the control group, which represents a loss of body-weight of almost 20 per cent., were however probably in a state of chronic under-nutrition. Now, the corrective influence on glucose tolerance of a few days' adequate carbohydrate intake has been shown in well-nourished subjects who had undergone a short period of fasting. The disturbance of carbohydrate metabolism in chronic under-nutrition may be different, and not so easily reversible. Indeed, animal experiments have shown that chronic under-nutrition

produces its own characteristic disturbance of carbohydrate metabolism (Halmi and Spirtos, 1956); and Lingjaerde (1956), from observations on mental patients, states that certain of them, especially acute schizophrenics and depressives, need relatively large quantities of carbohydrate for a relatively long time before glucose tolerance returns to normal. He suggests, as have others (Ström-Olsen, 1932; Robinson and Shelton, 1940), that the decrease in glucose tolerance found in certain mental illnesses is due to previous refusal to eat and its duration.

Data obtained during the present investigation are relevant to the problem. Although a greater percentage of depressions than controls gave a history of loss of weight and poor appetite before admission, the differences are not statistically significant. Such data, obtained from patients and relatives, are however of low reliability, and in view of the impairment of appetite known to occur in depression it remains probable that the decreased weight of the group of depressions is due to decreased food intake. The relationship of body weight and diet on the one hand to glucose tolerance on the other, within the group of depressions, is not however a simple one. The highly significant lower mean increment index in the four depressions with a decreased food intake during the four days before testing, suggests that the lower food intake may have been the cause of the lower glucose tolerance. When however the mean increment index (2.53) of the ten depressions with weights below the mean weight for the depressions, is compared with the mean index (2.27) of the nine with weights above the mean weight, no correlation between low body weight and low glucose tolerance is found. However, it remains possible that changes in body weight may correlate with simultaneous changes in glucose tolerance, but the data needed to establish this have not been obtained. At present, although both low glucose tolerance and loss of body weight are known to occur in depressions, a dependence of impaired glucose tolerance on loss of weight has not been shown, and both could be due to common underlying factors.

It has been stated that the most important factor determining the shape of the oral glucose tolerance curve in depressions is probably absorption from the alimentary tract (Greville, 1945). In the present investigation absorption was eliminated by giving the glucose intravenously. The results with the intravenous test indicate a delay in glucose utilization, and it has been shown that hyperglycaemia in depressions is correlated with inhibition of the hexokinase reaction by plasma from the same patient (Weil-Malherbe and Bone, 1951); this suggests that the hyperglycaemia is due to inhibition of the initial step in glucose metabolism. The inhibitory factor in the plasma is thought to be of hormonal origin, possibly from the adrenal cortex (Weil-Malherbe and Bone, 1951). It is of interest that in the rat liver a short period of fasting can likewise inhibit the hexokinase reaction (Wyshak and Chaikoff, 1953).

Other explanations relate the decrease in glucose tolerance in mental illness to emotion or "stress", the effect being mediated by the secretions of the medulla or cortex of the adrenal glands (Kooy, 1919; McCowan and Quastel, 1931; Holmgren and Wohlfahrt, 1944; Lingjaerde, 1956; Rames and Simon, 1955). Strong emotion, such as acute fear, is well known to cause hyperglycaemia, through increased secretion of adrenaline. The twelve patients in the present investigation judged to show symptoms of anxiety, had a somewhat better glucose tolerance than the seven who showed little evidence of anxiety (mean increment indices 2.58 and 2.11 respectively). But it could be argued that all patients were in the grip of an affect more powerful than anxiety, and

indeed it has been shown that among the various categories of mental illness, plasma adrenaline occurs in highest concentration in the depressions (Weil-Malherbe, 1955).

The data available provide no explanation for the significant difference in glucose tolerance between the groups of involutional and manic-depressive depressions.

SUMMARY

Glucose tolerance values and various physical and mental data were obtained from two groups: 19 depressions and 9 controls, of similar age distribution. An intravenous test which gave a numerical index for glucose tolerance was used.

A highly significant decrease in glucose tolerance and in body weight is found in the depressions compared with the controls. However, within the group of depressions, there is no correlation between glucose tolerance and body weight. Low food intake during the four days before testing (4 cases) is correlated with a low glucose tolerance. The glucose tolerance in 8 patients with involutional melancholia is significantly lower than in 7 patients with manic depressive psychosis. Explanations for these findings are discussed.

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ADDENDUM

Since the above was written, a paper has appeared in which is presented a convincing argument for regarding the total or absolute index as a better measure of glucose utilization than the increment index used above.* The absolute index is obtained by plotting log. absolute glucose concentration against time, and drawing the straight line through the values (above fasting level) after the 25th minute from the beginning of the injection; then, $K = \log_e 2/t_r$, as used for calculating the increment index. The absolute index has therefore been obtained (from the data) for each patient in the above investigation, and all the relevant values recalculated. Statistical analysis gives results which are in full agreement with those obtained using the increment index. The absolute indices for cases 1 to 19 are, respectively: 0.83, 1.20, 0.99, 0.89, 1.06, 1.49, 1.04, 0.94, 1.04, 1.12, 0.97, 1.31, 1.44, 1.46, 0.88, 0.91, 1.15, 1.01, 1.01; and for cases 20 to 28: 1.46, 1.28, 1.54, 1.56, 2.24, 1.37, 1.87, 1.47, 1.59; mean absolute index for depressions: 1.09, s.d. 0.16; and for controls: 1.60, s.d. 0.31.

REFERENCES

- CHAMBERS, W. H., *Physiol. Rev.*, 1938, **18**, 248.
 DUNCAN, L. J. P., *Q. J. Exp. Phys.*, 1956, **41**, 1, 85.
 EPSTEIN, A. A., and ASCHNER, P. W., *J. Biol. Chem.*, 1916, **25**, 151.
 FLETCHER, A. A., *Arch. Int. Med.*, 1922, **30**, 106.
 FREEMAN, H., RODNICK, E. H., SHAKOW, D., and LeBAUX, T., *Psychosom. Med.*, 1944, **6**, 4.
 GREVILLE, G. D., *Biochem. J.*, 1943, **37**, 17.
Idem, *Proc. Roy. Soc. Med.*, 1945, **38**, 671.
 HALMI, N. S., and SPIRTOS, B. N., *Amer. J. Physiol.*, 1956, **187**, 432.
 HENNEMAN, D. H., ALTSCHULE, M. D., and GONEZ, R. M., *Amer. Med. Ass. Arch. Int. Med.*, 1954, **94**, 3.
 HOLMGREN, H., and WOHLFAHRT, S., *Acta Psychiat. et Neurol.*, Scand., 1944, Suppl. 31.
 KOOY, F. H., *Brain*, 1919, **42**, 214.
 LINGJAERDE, O., *Acta Psychiat. et Neurol. Scand.*, 1956, Suppl. 106.
 MCCOWAN, P. K., and QUASTEL, J. H., *J. Ment. Sci.*, 1931, **77**, 525.
 MANN, S. A., *Ibid.*, 1925, **71**, 442.
 RAMES, E. D., and SIMON, W., *Arch. Neurol. Psychiat.*, 1955, **74**, 40.
 ROBINSON, G. W., and SHELTON, P., *J. Amer. M. A.*, 1940, **114**, 2279.
 SILVERSTONE, F. A., BRANDONBRENER, M., SHOCK, N. W., and YIENGST, M. J., *J. Clin. Invest.*, 1957, **36**, 504.
 STRÖM-OLSEN, R., *Lancet*, 1932, *i*, 128.
 WEIL-MALHERBE, H., *J. Ment. Sci.*, 1955, **101**, 733.
Idem and BONE, A. D., *ibid.*, 1951, **97**, 635.
 WYSHAK, G. H., and CHAIKOFF, I. L., *J. Biol. Chem.*, 1953, **200**, 851.

* IKKOS, D., and LUFT, R., *Acta Endocrin.*, 1957, **25**, 312.

BASAL AND SLEEPING METABOLIC RATES IN PSYCHIATRIC DISORDERS

By

P. J. DALLY, M.B., M.R.C.P., D.P.M.

Registrar

Department of Psychological Medicine, St. Thomas's Hospital, S.E.1

Formerly Registrar

St. Ebba's Hospital, Epsom

THE part played by the endocrine system in psychiatric illness is still controversial. Mental disorders may occur at the menopause or in the puerperium, accompany thyroid or pituitary dysfunctions, or appear in adrenocortical diseases. Castration in men and women may be followed by marked personality changes. But often there are little or no mental changes, and there is certainly no clear-cut mental accompaniment of specific endocrine diseases. It is probable that mental symptoms, when they do occur, are dependent upon the basic personality structure, not upon a particular diseased gland. It is also likely that glandular dysfunction in turn can be brought about by mental stress or disorder. In other words, the two systems function together and should not be artificially separated.

The activity of the thyroid gland is frequently questioned. Severe cases of myxoedema can easily be diagnosed but less obvious forms of hypothyroidism may be very difficult to identify, and often impossible if agitation is present. Similarly, patients with severe thyrotoxicosis present little difficulty in diagnosis, but milder cases may be impossible to distinguish from simple agitation. To add to the diagnostic difficulty it has been found that a proportion of patients with anxiety symptoms, particularly women, have diffusely enlarged thyroid glands and Brody (1949) has suggested, on the basis of blood iodine studies, that some of the symptoms of anxiety are produced by slight excess of thyroid. Fraser (1953) has remarked on the tendency of neurotic patients to have a relatively high uptake of radio-active iodine.

Thyroid activity may be measured by mean of various tests, but the simplest and probably the most reliable method at the present time is to estimate the basal metabolic rate (B.M.R.) of the patient. The basal state is defined as the amount of energy required just to maintain body processes when the person is at complete mental and physical rest, and is some twelve or more hours from the time of his last meal. A condition of complete mental and physical rest is essential, otherwise the test merely measures the metabolic rate of the person at that particular moment; the result will be misleading if regarded as the basal metabolic rate. In anxious patients, particularly those admitted to mental hospitals, complete mental rest is usually impossible to attain. Measurements of the B.M.R. of such patients in the past have been unreliable and often of little value either for diagnosis or for following the course of treatment. Consequently other methods, such as radioactive iodine uptake and excretion rates, and the level of protein-bound iodine in the serum, where facilities for such estimations exist, have come to be regarded as far more reliable than the B.M.R. However, it is important to realize, as Meckstroth (1952) has pointed

out, that these different tests cannot be compared among themselves as they each measure a different aspect of thyroid metabolism. The B.M.R. measures the total body metabolism and therefore represents the end result of many oxidative processes, only one of which is due to thyroid hormone.

At St. Ebba's Hospital estimations of the B.M.R. with a Benedict-Roth machine have been carried out on all types of patient for the past twenty months. Initially the tests were done without sedating the patient.

NON-SEDATED B.M.R.—METHOD AND RESULTS

The patient was given a trial run on the day preceding the test and told of the procedure and purpose of the test. No nourishment was given after 8 p.m. but short-acting hypnotics were allowed for the night. On rising at 7 a.m. the patient emptied his bladder and went to the "B.M.R. room" where he lay quietly in bed until 9 a.m. when the test was done. The temperature of the room was kept at 20°C.

During the test, whenever possible, the patient's consumption of oxygen over ten minutes was measured. But sometimes he became very agitated after a few minutes, his respirations increased in rate and amplitude, and the rate of oxygen consumption rose rapidly. Repetition of the test next day invariably produced the same response. When a patient complained of claustrophobia he usually found the procedure intolerable from the start. Psychotic patients sometimes refused to co-operate because of their delusions, or laughed and talked in the middle of the test, but in general were easier to manage than those with anxiety states.

The B.M.R. was calculated from the rate of oxygen consumption, in terms of calories per square metre of the patient's surface area per hour, and expressed as a percentage of the mean normal of the standards of Robertson and Reid (1952). These standards are some 10 per cent. lower than other standards, such as those of Boothby *et al.* The normal range for Robertson and Reid standards is generally taken as ± 14 per cent.

The results obtained in 189 consecutive estimations of the B.M.R. of unsedated psychiatric patients showed a very wide range:

64 were +15 per cent. or more

11 were -15 per cent. or more

40 per cent. were therefore outside the normal range of ± 14 per cent., 34 per cent. being above normal. Of this total, 4 patients only were clinically thyrotoxic, and 1 was clinically hypothyroid. These results may be compared with the work of Robertson and Reid who, in the course of determining their standards, found that 9 per cent. of their subjects (all presumably mentally healthy) gave initial readings 15 per cent. or more higher than those on the second day.

These results confirm the belief that the B.M.R., estimated under non-sedated conditions on anxious patients, is of little use as an index of thyroid activity. For the test to be of value it is necessary to produce basal conditions by artificial means, that is by inducing sleep. Possibly in a proportion of nervous patients, daily estimations of the B.M.R. such as Robertson and Reid carried out would eventually give a truly basal reading, but in practice this is far too time-consuming. This method was tried on several very tense patients with high B.M.R.s but as no appreciable drop occurred after six readings on each, it was abandoned.

Bartels (1949), using thiopentone (Pentothal), found that there was a drop in the mean B.M.R. of 17 healthy people from +4 per cent. (+16 per cent. to -10 per cent.) to -9 per cent. (+9 per cent. to -26 per cent.), while little drop occurred in proven cases of hyperthyroidism. Rapport (1951) used pentobarbitone (Nembutal) and obtained an average fall from +13 per cent. to -2 per cent. in 74 euthyroid patients, while 74 hyperthyroid patients all remained above normal, showing an average fall from +46 per cent. to +33 per cent. More recently Fraser and Nordin (1955) have described their method of measuring the B.M.R. during sleep, and emphasized that this is the only satisfactory technique in nervous patients. It was therefore decided to measure the B.M.R. of patients under sleeping conditions, and see whether more reliable results could be obtained.

METHOD

Two methods of producing artificial sleep were tried:

Oral Method

Nembutal, gr. 3 was given by mouth hourly for three hours. This method was clumsy and unreliable compared with the intravenous method (described below). There was no means of controlling the depth of sleep. Some patients slept deeply after 3 gr., while others awoke during the test after 9 gr. 34 estimations by this method were made, but it has now been abandoned in favour of the intravenous technique. Tests were carried out in the non-sedated as well as the sleeping state, and the unreliability of the method is shown by the results:

Of 34 non-sedated patients 18 were +15 per cent. or more

Of 34 "sleeping" patients 9 were +15 per cent. or more

1 patient only was clinically thyrotoxic.

Intravenous Method

Nembutal was injected intravenously, 1 c.c. (50 mgm.) initially and then 0.5 c.c. per minute until sleep was produced. The average dose in adults was found to be 200-300 mgm. This took about ten minutes to give, and allowed initial agitation time to fade. The advantage of this method over oral sedation was that the operator could choose his own time and was able to produce light sleep easily and consistently. There was little or no risk of producing a dangerous level of anaesthesia provided the rate of injection was not increased. (It was important, however, to make sure that the patient really was asleep, otherwise false results were sometimes obtained.) Oxygen and Coramine were always at hand, but there was no occasion for their use.

The mouthpiece was inserted as soon as the patient was asleep. It was sometimes necessary to hold up the jaw, or lightly hold the lips around the mouthpiece, particularly with edentulous patients, but this procedure did not disturb the patient's sleep. A satisfactory record of oxygen consumption over 10 minutes was nearly always possible. The respiration rate generally dropped slightly but no more than normally occurs in sleep, but when too large a dose of Nembutal was inadvertently injected there sometimes occurred an increase in the rate. In such an event there was usually an over-large fall in the oxygen consumption. The pulse, when raised initially from anxiety, fell to within normal limits or otherwise remained unchanged. The patient tended to be

drowsy for several hours, but this period could be shortened by giving 10 mgm. dexedrine on completion of the test. Occasionally, if food was taken before the drowsiness wore off, the patient felt sick and sometimes vomited.

RESULTS

Eighty-one sleeping metabolic rates (S.M.R.) have so far been estimated. In all but two of the patients it was possible to determine the B.M.R. before inducing sleep.

In 79 non-sedated patients, 42 were +15 per cent. or more and 2 were -15 per cent. or less.

In 81 sleeping patients, 9 were +15 per cent. or more and 5 were -15 per cent. or less.

Four of these patients were clinically thyrotoxic; 1 patient was thought to have Cushing's Syndrome.

DISCUSSION

In this small series 53 per cent. of non-sedated patients had a B.M.R. above normal. When the sleeping metabolic rates of these same patients were estimated only 11 per cent. were still above +14 per cent. The question that arises immediately is to what extent this sleeping state is equivalent to the basal state. Some authorities argue that barbiturates depress metabolism below the basal level. Certainly if so large an amount of barbiturate is given that surgical anaesthesia is induced then the metabolism will fall below basal level. But when nembutal is given intravenously and slowly as described above a state of light sleep can invariably be produced. The evidence accumulated during this investigation suggests that the sleeping metabolic rate under these conditions is, for practical purposes, similar to the basal metabolic rate. Thus, 35 patients had B.M.R.s within the normal limits of ± 14 per cent., the mean being +5 per cent. (+14 per cent. to -7 per cent.). S.M.R.s were then estimated and the mean was found to be -1 per cent. These figures include, however, two patients who were hypersensitive to nembutal and one (later proven) hypothyroid patient whose agitation had lifted the B.M.R. to within the normal range. If these patients are omitted the mean S.M.R. is then +0.7 per cent. (+12 per cent. to -13 per cent.). When one considers that the B.M.R. may have been slightly raised anyway, the size of this fall, from +5 per cent. to +0.7 per cent., is small enough to suggest that the S.M.R. is synonymous with the B.M.R.

It was mentioned above that the B.M.R. represents the end result of many oxidative processes. Nervous factors obviously affect these processes and, as already shown, account for the high proportion of B.M.R.s above the normal range in non-sedated patients. The S.M.R. measures the metabolic rate in the absence of these nervous factors and it would therefore seem that the difference between the B.M.R. and S.M.R. should give a quantitative measure of the patient's tension.

Twenty-eight patients with clinical signs and symptoms of anxiety had a mean B.M.R. of +27.7 per cent. (+50 per cent. to +15 per cent.), falling during sleep to a mean of +4.7 per cent. (+22 per cent. to -12 per cent.). In eight of these patients the metabolic rate was estimated at varying intervals after induction of sleep. The results showed a progressive increase in the metabolic rate as the effect of nembutal wore off and twenty-four hours later the B.M.R. approached, and in one case exceeded the initial non-sedated level.

	B.M.R. Non-sedated (Per cent.)	S.M.R. (I.V. Nembutal) (Per cent.)	3 Hours Later (Per cent.)	6 Hours Later (Per cent.)	24 Hours Later (Per cent.)
1 ..	+47	+14	+14	+19	+40
2 ..	+33	+22	+29	+29	+27
3 ..	+35	+9	+21	+21	+30
4 ..	+29	+16	+12	+19	+25
5 ..	+24	+7	+13	—	+21
6 ..	+38	-10	-8	+10	+32
7 ..	+39	-12	+21	+20	—
8 ..	+22	+1	+20	—	+26

These figures confirm the idea that the difference between the B.M.R. and the S.M.R. represents, in quantitative terms, the tension of each individual. This suggests that it may be possible to follow and measure a patient's progress under various forms of treatment.

Several patients have had their B.M.R. and S.M.R. repeated at monthly intervals in an attempt to confirm this possibility.

	B.M.R. %	S.M.R. %	Tension	
<i>Mrs. A.</i>				
January ..	+24	+9	15	Very tense on admission. Recovery followed sedation and superficial psychotherapy.
February ..	+8	+6	2	
<i>Mrs. B.</i>				
March ..	+26	+10	16	Very tense and depressed. Slow improvement until chlorpromazine was given.
April ..	+22	+7	15	
May ..	+10	+8	2	
<i>Mrs. C.</i>				
June ..	+30	+3	27	Very tense. Failed to improve whilst in hospital, in spite of various treatments.
July ..	+30	-3	33	
August ..	+24	+16	8	
<i>Mrs. D.</i>				
August ..	+23	+13	10	Became increasingly tense whilst in hospital and finally discharged herself against advice.
September	+47	+14	33	

In the cases of Mrs. A, B and D the tension corresponded well with the clinical progress. In Mrs. C's case there was a marked increase in the S.M.R. and it seemed likely that a vicious neuroendocrine circle had been set in motion.

Nine of the S.M.R. estimations of this series were ≥ 15 per cent. or more. Four of these were clinically thyrotoxic and one was an example of Cushing's Syndrome. The remainder showed severe anxiety symptoms. Each had a palpable smooth thyroid but none was clinically thyrotoxic and the radioactive iodine uptake and excretion rates were within the upper ranges of normal. None of these patients responded to sedation and psychotherapy. Two had rostral leucotomies and improved markedly (unfortunately the post-operative B.M.R. was not estimated), one was recommended for leucotomy but discharged herself, and the last was discharged to a general hospital for a hysterectomy (for uterine fibroids) and has not subsequently been traced. It seems therefore that a S.M.R. above the normal range, in the absence of thyrotoxicosis or other causes of increased metabolism, is of bad prognostic omen. A vicious circle seems to become set up in such patients which may need to be broken by leucotomy.

The four patients with thyrotoxicosis had S.M.R.s above normal, confirming the statement of Fraser and Nordin (1955) that the metabolic rates of hyperthyroid patients during sleep continue above the normal range. The actual values obtained were:

				B.M.R. Per cent.	S.M.R. Per cent.
W.	..	..	..	+64	+44
X.	..	..	..	+29	+16
Y.	..	..	..	+36	+39
Z.	..	..	..	+36	+21

By contrast two women and three men had S.M.R.s below -14 per cent. Two of the men proved to be abnormally sensitive to nembutal and the low figures were almost certainly a reflection of this. The third man had a B.M.R. of -16 per cent. which fell to -23 per cent. during sleep. He was a schizophrenic and was later shown to be very insensitive to thyroxine. Very low B.M.R.s have quite frequently been described in apparently normal people (Booyens, 1957), and Knowland (1955) discusses a group of euthyroid people who have B.M.R.s 20–30 per cent. below average, unresponsive to thyroid extract. Of the two women, one was a severely agitated depression who refused to co-operate and whose B.M.R., when awake, would have been wholly unreliable. She was very thin and this may have lowered her basal metabolism. The other woman had had a thyroidectomy eighteen months previously and was showing marked anxiety symptoms on admission. The B.M.R. at this time was -6 per cent. and the S.M.R. -20 per cent. This last figure was thought to represent the actual index of her thyroid activity. The interesting point about this patient is that she was initially thought to have a recurrence of thyrotoxicosis.

CONCLUSION

1. Valid estimations of B.M.R. of psychiatric patients are possible, but it is essential to carry out such estimations under sleeping conditions if the patient is tense or agitated.
2. Sleep is best induced by giving nembutal intravenously. Oral nembutal is unsatisfactory for this purpose.
3. The sleeping state is synonymous with the basal state.
4. The difference between the basal metabolic rate and the sleeping metabolic rate is a quantitative measure of tension for each patient. This measure can provide an objective index of progress or deterioration under various treatments.
5. The sleeping metabolic rate of thyrotoxic patients remains above the normal range.
6. A sleeping metabolic rate that remains above the normal range, in the absence of thyrotoxicosis or other causes of increased metabolism, is of bad prognostic significance. Such patients may require leucotomy to relieve the tension.
7. The sleeping metabolic rate may reveal cases of hypothyroidism disguised by anxiety symptoms when awake.

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REFERENCES

- BARTELS, E. C., *J. Clin. Endocrin.*, 1949, **9**, 1190.
 BOOYENS, J., and McCANCE, R. A., *Lancet*, 1957, *i*, 225.
 BRODY, E. S., *Psychosom. Med.*, 1949, **11**, 70.
 FRASER, R., *Quart. J. Med.*, 1953, **22**, 99.
Idem and NORDIN, B. E. C., *Lancet*, 1955, *i*, 532.
 KNOWLAND, G. S., *et al.*, *J. Clin. Endocrin.*, 1955, **15**, 1354.
 MECKSTROTH, C. V., *et al.*, *J. Clin. Endocrin.*, 1952, **12**, 1373.
 RAPPORT, R. L., *et al.*, *J. Clin. Endocrin.*, 1951, **11**, 1549.
 ROBERTSON, J. D., and REID, D. D., *Lancet*, 1951, *i*, 940.

REACTIONS OF LOW-GRADE MENTAL DEFECTIVES TO PAIN

By

E. STENGEL, M.D., M.R.C.P.

*Professor of Psychiatry
University of Sheffield*

A. J. OLDHAM, M.D., D.P.M.

*Consultant Psychiatrist
Cane Hill Hospital, Coulsdon, Surrey*

and

A. S. C. EHRENBERG, B.Sc.

*Group Statistician
Attwood Statistics, Ltd.*

IN an earlier study (Stengel *et al.*, 1955), reactions of various types of mental patients to eleven different painful and noxious stimuli were described. Two groups of 54 females and 43 male low-grade defectives were included in the original investigation, but it was felt at that time that the results needed further amplification before they could be usefully commented upon. In this earlier investigation only one of us actually carried out the tests, the reliability of which, therefore, was not clearly established. Furthermore, in the original testing of low-grade defectives no assessment of "Overt Reactivity" was made. As the relationship between this factor and pain reactivity in the other patient groups constituted an important finding of the study, we came to the conclusion that it would be necessary to retest some of the low-grade defective patients with these points in mind. This seemed all the more desirable as it is widely assumed that mental defectives in general, and especially low-grade mental defectives, are grossly deficient in sensitivity to pain.

Experimental Procedures. The differences from the experimental procedures set out in detail in the first publication (*Journal of Mental Science*, 1955, **101**, 52), to which the reader is referred, are as follows:

Ten of the original male and 10 of the original female low-grade defectives who had first been tested in February, 1952, were each retested by two of us (E.S. and A.J.O.) and by Dr. F. Letemendia in April, 1955, using the same tests, namely:

1. Pin pricks to right and left palms
2. Pin pricks to right and left cheeks
3. Pin pricks to right and left thighs
4. Clap "tests" on right and left sides

As in the original investigation the movement and wincing responses were scored on a scale ranging from 0 to 4. As distinct from the original investigation, the three observers independently scored each patient's responses, but one of us (A.J.O.) actually applied the test stimuli in each case. The order of the four tests given, and of the right and left applications, were randomized for each patient instead of being kept the same. In addition, each of the three observers

made independent assessments of the patient's "Overt Reactivity" from the ward sister's report of each patient's behaviour over the preceding twenty-four hours. As in the original work, "Overt Reactivity" (O.R.) was assessed on a three-point scale, i.e.:

1. Increased O.R.; patient grossly hyperactive and/or tense.
2. Decreased O.R.; patient markedly hypoactive and/or apathetic.
3. Intermediate O.R.; cases not classified (1) or (2) above.

In this way it was intended to check the reliability with which the scoring system could be used, and to ensure that scores for a particular test were not systematically influenced by the preceding tests. (In particular, it was noted in the earlier paper that the almost complete agreement then between the right and left scores was probably spurious.)

RESULTS

1. *The Three Assessors.* The scoring by the three doctors was in good agreement. Their average scoring levels agreed closely, showing that there was no tendency for one doctor to score higher than the others. But more than this, in over 40 per cent. of the scoring of the painful tests the three doctors agreed completely and in the same number of cases two agreed and the other differed by one unit. In only 6 per cent. of the cases did all the doctors differ, and even then practically always by at most two units. The general level of agreement for scoring the clap tests was very similar, although the percentage of complete agreement was just under 30 per cent. The reason for this difference from the painful tests was that for 30 per cent. of the latter, all three doctors scored the highest possible score, 4, which did not occur for the clap test.

It follows that reactions to these painful and noxious stimuli can be assessed by different doctors in a reliable manner, a point which had not been established in the earlier investigations.

2. *Test-Retest Results.* The agreement of different assessors in scoring the same responses is of course not enough to establish the whole test procedure as a reliable one in the usual sense. It may be that although the responses can be judged consistently they are not in any way themselves consistent, e.g., from one occasion to the next, either because the administration of the stimulus is not done in a sufficiently controlled manner, or simply because the responses to such stimuli are not consistent.

However, the test-retest results discussed in the earlier paper showed that this is not the case, and this finding has been extended by the fact that the results obtained now for the 20 low-grade defectives agreed very closely with those obtained for them when they were tested 38 months previously. For example, the average scores on the three painful tests varied in only 3 out of the 20 cases by more than 1 unit from the previous results.

In all the work so far the test stimuli have been administered by the same doctor (A.J.O.), so that it is not yet known what consistency of response can be achieved when the stimuli are administered by different persons.

3. *Right and Left Administration.* One feature of the earlier results which seemed to have a dubious validity was the comparison of the responses when each test was administered first on the right and then on the left. Each pair of scores had, in almost every case, been identical, and this could well have been a spurious finding as the right and left stimuli were applied consecutively. However, even with the randomized design adopted close agreement was found. Thus,

well on half the pairs of right and left scores were identical, and such differences as were observed usually did not exceed 1 unit and showed no tendency to favour either the right or the left. Each of the three doctors scored right different from left in a similar number of cases, and there was even some tendency for them to score right and left agreements or differences in the same cases. It follows that small but true right and left differences in response may have occurred, though if they did, they may of course have been due to the administration of the stimuli or to a fluctuating responsiveness of the patient rather than to any more fundamental difference in reaction.

4. *Movement and Wincing Responses.* Two modes of response, namely, Withdrawal Movement (M) and Wincing (W) were assessed for each of the four tests. For the three Pin-Prick Tests and the M responses tended to be on average $\frac{1}{4}$ to $\frac{1}{2}$ unit higher than those for W, but on the Clap Test there was no such difference. This small average difference was not a general one but was mainly due to 5 patients whose average M scores were more than 1 unit higher than those for W. But it may be noted that for the medium-grade defectives of the earlier study a similar difference was found for the comparable Pin Prick Tests.

5. *Individual Differences.* Different patients varied greatly in their responses to the tests, all scores from 0 to 4 in fact being used, and whilst some patients had average scores as low as $\frac{1}{2}$, others were given scores of 4 for all or practically all responses. This latter feature shows that there may be scope for improving and extending the assessment of responses at the upper end of the present scale.

On average, female patients scored lower than the males by about $\frac{1}{2}$ unit. Because of the large individual differences this result is highly unreliable but should perhaps be mentioned, because differences in the same direction were found in every diagnostic group in the previous study.

6. *Test and Group Differences.* The results of three Pin Prick tests were on the whole very similar, but differed from those for the Clap Test. Thus the average scores (right and left, male and female, all patients) for the Palm, Cheek and Thigh Tests were 2.6, 2.8, and 2.6 units respectively, whereas that for the Clap Test was 1.4 units. Because of the very large individual differences and the relatively small number of patients tested, these average scores cannot be thought of as indicating the average level of response for low-grade defectives generally. And the fact that the average score on the Palm Test was higher than for any of the other diagnostic groups in the earlier study must also be interpreted with reserve. Nevertheless, it is clear that the low-grade defectives responded as much if not more to painful pin pricks as did our other defective and psychotic patients. Furthermore, whereas the overall scoring level of all the tests is subject to large variation, the difference between the two types of test, i.e., painful (Pin Prick) and threatening (Clap Test) is much more reliable. Thus the average difference between the Palm and Clap Test here was 1.2 units, as compared with consistent differences of about 0.3 units for the same two tests for all the diagnostic groups in our previous study. The inference that the low-grade defectives reacted less strongly to threatening than to painful stimuli is thus inescapable.

7. *Overt Reactivity.* As found previously for all the diagnostic groups there was a definite relationship between "Overt Reactivity" and the test scores. The average differences in scores between the hyper- and hypo-reactives was 1.3 units, as may be seen from Table I.

TABLE I

				Overt Reactivity		
				+	0	-
No. of patients	..	..	..	8	10	2
Average test score	..	..	..	2.9	2.1	1.6

8. *Aetiological Grouping of Low-Grade Defectives.* An attempt was made to classify the 97 low-grade defectives of our original study into aetiological groups by a careful study of the case-material available. A very large number of aetiological factors were assembled for many cases while in some patients no such factors were found. The picture was complicated by the fact that in a number of cases several different aetiological factors appeared to co-exist, such as congenital malformations and acquired organic cerebral disease, so that composite groupings had to be made. When these were compared with the patients' test responses no evidence of any association was forthcoming.

9. *Low-Grade Defectives with a Propensity to Self-Injury.* In our earlier study a number of low-grade defective patients were noted persistently to injure themselves and in doing so they showed no outward indication of feeling pain, rather some evidence of pleasure. The degree of self-injury sustained by these patients was roughly classified on a two-point scale, + slight with only superficial self-inflicted wounds, and ++ severe with deep and extensive self-injuries. The number of such patients observed is shown in Table II. Of all the male and female low-grade defectives tested by us therefore, just under one quarter were subject to this type of behaviour.

TABLE II

				Total No.	No. of "Masochists"	
					+	++
Female patients	..	..	..	54	8	2
Male patients	..	..	..	43	7	5

When the test responses of these "masochistic" patients were scrutinized it was found that those graded + scored on average $\frac{1}{2}$ unit lower, while those graded ++ if anything more than $\frac{1}{2}$ unit lower than the "non-masochistic" patients. The evidence, so far as it goes therefore, tends to indicate that these "masochistic" low-grade defective patients responded slightly less to painful and threatening stimuli than those not subject to such masochistic tendencies.

DISCUSSION

Most of the results speak for themselves and do not require special comment. The test procedure employed in this group, which presented a refinement on the techniques employed in the earlier study, revealed slight differences between the reactions to stimuli applied on the right and left side of the body, but these appeared to be in the nature of individual variations, rather than due to a more general difference in reactivity. Low-grade mental defectives showed the same tendency to marked individual differences in the degree of reactions as was found in all other types of patients suffering from abnormal mental states.

The low-grade mental defectives tested in this research reacted to the Clap Test, which is both an auditory and a threatening stimulus, much less than to the painful stimuli, while patients belonging to the other diagnostic groups had

reacted only slightly less. This seems to suggest that low-grade defectives are less responsive to threatening stimuli which are not associated with pain. Clinical observation seems to bear this out. However, the difference in the reactions to those tests may be partly due to conditioning by the ward environment which tends to be much more permissive with regard to noise and motor activity than with regard to infliction of pain.

This investigation clearly demonstrated that the average low-grade mental defective reacted to pain inflicted from outside in the same way as other subjects, i.e., there were considerable variations which were related to individual differences in psychomotor behaviour. E. Popper's (1920) suggestion referred to in our first publication, that the apparent diminished pain sensitivity of mental defectives might be related to their torpidity and apathy was thus confirmed. Possibly, some of the conditions observed by earlier psychiatrists were institutional artefacts produced by excessive sedation and greater restriction of activity than are the rule today. Such factors might often have reduced psychomotor activity and reactivity to pain.

The most striking result of this investigation was the finding that low-grade mental defectives who indulge in self-injuries tended to react to pain inflicted on them from outside only slightly less than other subjects. They displayed, therefore, a marked discrepancy in their reactions between self-inflicted and other painful stimuli. The motivation of their impulse to self-injury remains obscure. The satisfaction which many appear to derive from inflicting pain on themselves is suggestive of an underlying masochistic gratification, although no manifest sexual excitement accompanying the self-infliction of pain was observed in any patient. At any rate, the difference between the reactions to painful stimuli and those administered by others is of fundamental significance from the psychological and physiological points of view. There are other examples of such discrepancies in animal and human behaviour: the one nearest to that described here is perhaps the difference in the reactions to tactile stimuli which are experienced as tickling when administered by another person and devoid of this quality when self-administered.

Insensitivity to pain was not observed in any of the patients of this series. This behaviour pattern has occasionally been noted in mental defectives of various grades though not as a rule associated with a tendency to self-injury and mutilation. It has, of course, also been observed in non-defective subjects. The tendency to self-mutilation cannot therefore as a rule be based on insensitivity to pain congenital or acquired.

CONCLUSIONS

Low-grade mental defectives tested for their reactions to painful and other noxious stimuli showed a range of variability similar to that of other subjects, except that they tended to react to a stimulus which was threatening and auditory less than to painful stimuli. The degree of their reactions showed a positive correlation to the degree of Overt Reactivity. Low-grade defectives with a propensity to self-injury reacted to pain inflicted from outside only slightly less than other subjects.

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SECONDARY ACUTE LETHAL CATATONIA

By

R. S. FERGUSON, M.D., D.P.M.

St. Nicholas Hospital, Gosforth, Newcastle upon Tyne

At various times attempts have been made to establish a specific pathological entity "acute lethal catatonia". Stauder and Scheid (quoted by Mayer Gross *et al.*, 1954) among the older writers have collected a number of fatal cases and more recently Locher (1941) has suspected qualitative difference in the type of illness of those mental patients who die suddenly in an acute catatonic state. However the general view nowadays seems to be that death is due to exhaustion or intercurrent illness. A fatal outcome is very unusual in current practice perhaps because of improved therapy. The symptoms of acute catatonic schizophrenia are so striking that a determined quest for a biochemical basis for the whole schizophrenic process has been sought in this, its most dramatic manifestation. Success in this direction has often been claimed (e.g. Fischer, 1953; Rieder, 1954; Gjessing, 1953, *inter alia*) but has also frequently been disputed (Georgi, Rieder and Weber, 1954). The following case is reported because the symptoms seemed to be clearly related to physiological change. It tends also to throw further doubt on the entity of acute lethal catatonia of Locher. Interesting EEG findings and characteristic response to amytal are also noted.

CASE REPORT

A woman aged 38, was admitted to mental hospital on 25 February, 1956. She was married, a housewife and lived at home with her husband and five children. She had been treated as an in-patient at a near-by general hospital for the previous six days as a middle lobe pneumonia and altogether had been ill for ten days. She had been treated at home by her own doctor for respiratory infection for the first four days but admission to hospital became advisable because of inadequate nursing care at home. She was found to have extensive physical signs in the chest and X-ray on 20 February, 1956 showed an opacity in the right lower zone which looked like consolidation of the middle lobe (radiologist's report). She is described as being apparently in an extremely toxic state, mildly cyanosed, rather confused mentally. Haemoglobin 11.4 gm., leucocytes 11,600 including 80 per cent. polymorphs. A series of agglutination and complement fixation tests were all negative (salmonella, Br. abortus, influenza, psittacosis, "Q" fever). Her respirations were 30-36 per minute and the rhythm was irregular. She was nursed in an oxygen tent and given penicillin 500,000 units stat. and 250,000 six-hourly thereafter, and vitamins A, B, C, D, E, K, P. Although considerable improvement took place in her lung condition, mental changes became apparent. She was apprehensive and detached, she would start sentences without finishing them, was unable to give any account of herself, and gradually became completely inaccessible and mute. She was noted to have general muscular hypertonus but such examination of the central nervous system as the patient allowed showed no focal signs. Eventually a period of excitement and motor overactivity greatly disturbed the ward and the patient was removed to observation at a mental hospital. ? Schizophrenia, ? toxic confusional state, ? encephalitis.

Condition on Admission

Shapiro (1956) has classified catatonic symptoms. The following description of the patient's condition is in accordance with his schema.

A. Hypokinetic phenomena:

- (1) Diminished motor phenomena
 - (a) Greatly retarded impulse to movement and action.
 - (b) Maintenance of motionless posture for long periods.
 - (c) Mute and stuporose.
 - (d) Mask-like facies.
 - (e) Grimacing and gesturing.
 - (f) Fixed blank stare.
 - (g) Sluggish rigid musculature.

- (2) Catatonic phenomena
 - (a) Refusal of food and need to be fed by others.
 - (b) Refusal to dress and undress.
 - (c) Incontinence.
 - (d) Absence of reaction to pin-prick.
- (3) Physiological negativism
 - (a) Active negativism.

B. Hyperkinetic phenomena

- (1) Irregular hyperkinesis: Unorganized activity.
Active negativism.

C. Autonomic phenomena

- (1) Respiratory changes—irregular respiration.
- (2) Acrocyanosis.
- (3) Hyperidrosis.

Of the total aggregate of 25 catatonic phenomena described by Shapiro therefore, this patient showed 17—a very high proportion in such a protean disease. She was a great nursing problem, holding herself rigid, actively resisting all care and attention and refusing food. The rigidity was extreme but plastic. Even tube feeding was difficult because of the patient's negativism. Breathing was laboured, temperature 100, pulse 100, respiration 32. She appeared to resist even her own cough reflex with the result that her cough was completely ineffectual. She did not trouble to cough up sputum as she easily could have done. "Estopen" 500,000 units daily was continued, and she was given 100 c.c. Parentrovite intravenously daily from 26–29 February, along the lines recommended by Gould (1954).

Response to Amytal

In this state of akinetic mutism some awareness of the environment was certainly present for negativism was of an active kind. The effect of intravenous amytal was therefore tried (15 gr. in 20 c.c. water). After 3–4 c.c. the patient at once relaxed, focused her attention and the following conversation ensued: "Do you know where you are?" "Yes." "Where?" "In hospital." "Which hospital?" "I don't know." "Well, it's in Gosforth." "I like the — best." "How many children have you?" "Five." "What is your husband's name?" "John Edward." "Where does he work?" "In the factory—ten pounds a week!" Here she smiled pleasantly and her mood became quite cheerful. Soon she began to cough violently, it seemed that the cough reflex was now able to operate more effectively. She stated that she was thirsty and took her first voluntary meal since admission. She asked for and was given the bedpan. For the rest of that evening she talked a good deal of confused and delusional material, but when her husband visited she asked after the children, and she asked for and drank milk. She had 6 c.c. amytal in all. Next morning she had reverted to her mute resistive state.

EEG

1 March. The patient was in a state of coma-vigil, but still showing active negativism. For example, when asked repeatedly to close her eyes, she kept them widely open, but when asked to open her eyes she would close them for up to half a minute at a time. Satisfactory recordings were obtained, showing a stable and mature alpha rhythm at 10 c.p.s. but paroxysmal slow activity at 3–5 c.p.s. occurred freely throughout the record (Fig. 1) having a generalized distribution, and suggesting a central origin.

2 March. This record was very similar to that of the previous day with a general all round reduction in amplitude. The patient was notably weaker.

3 March. Although the patient was by this time desperately ill, this EEG is the most "normal" of the three. The alpha rhythm is not yet disorganized, and the paroxysmal slow has disappeared. At this point the patient no longer displayed such clear cut active negativism.

Fatal Issue

Lumbar puncture on 1 March showed a clear fluid not under increased pressure. Cells 1/3 per c.mm. Globulin positive, protein 40 mg. per cent., chlorides (NaCl) 900 mg. per cent. Lange 0000000000. Blood sugar 110 mg. per cent. Blood urea 85–90 mg. per cent. The patient's course was steadily downhill and she died on 7 March.

Post-mortem report (Dr. B. E. Tomlinson). Death was probably due to acute pulmonary oedema precipitated by bronchopneumonia. The brain showed deep congestion over the entire cortex and there was a fairly marked tentorial pressure line over the uncus on each side. The vessels at the base of the brain were normal. There was no cerebellar pressure cone and no abnormality was revealed by a single horizontal cut through the cerebral hemispheres, exposing the lateral ventricles. Histological examination of numerous brain sections shows no evidence of encephalitis. Sections have been examined from the frontal, temporal, occipital and parietal lobes, from the uncus, thalamus, lentiform nuclei, hypothalamus, pons, medulla and aqueduct and beyond severe congestion with a very occasional pericapillary haemorrhage there is no significant abnormality. The meninges are also normal in appearance. The severe bronchopneumonia is confirmed, as is the fibrosis and

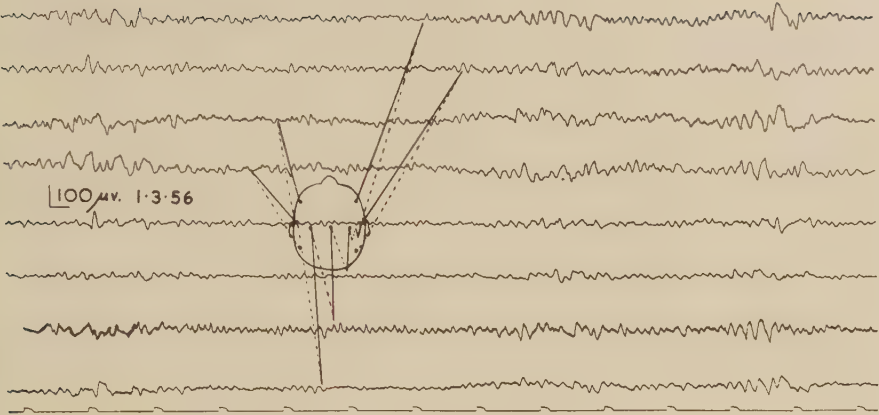


FIG. 1.—During stage of “active” negativism.

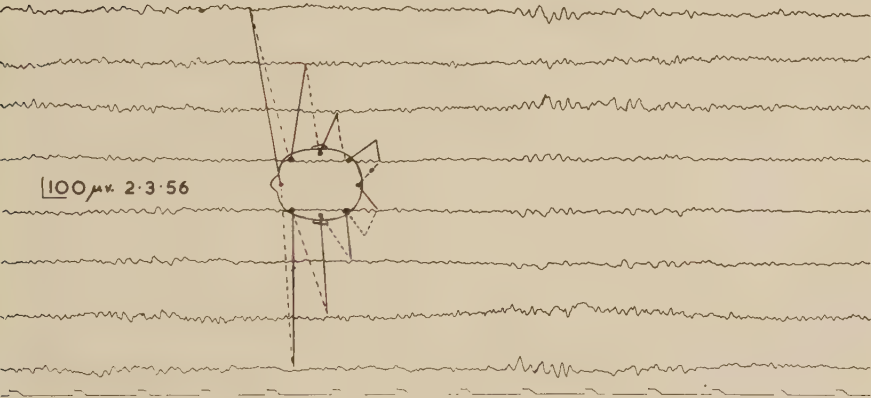


FIG. 2.—The next day. Negativism present but not so marked.

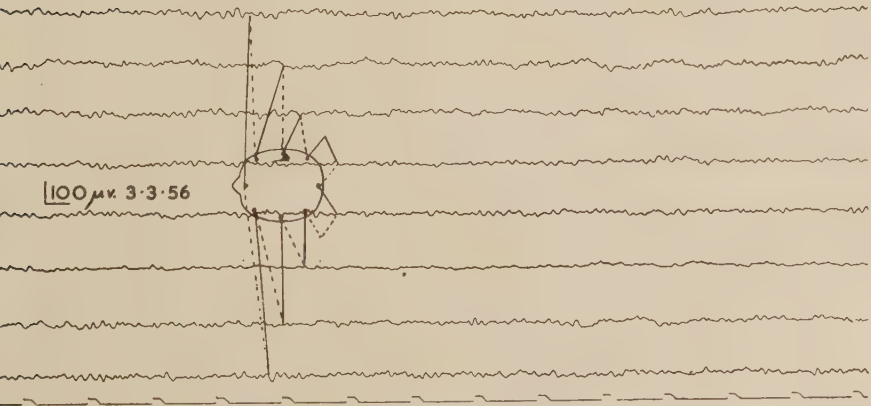


FIG. 3.—The following day. Patient extremely ill. Four days before death.

mild inflammatory change around the diverticula in the colon. The liver, heart, and pancreas and bone marrow and suprarenals show no significant abnormality. There is some arteriosclerosis in the kidney, and in the pituitary several small colloid cysts in the pars intermedia. No adenoma is present.

Previous History

The patient suffered every winter from severe coughs and colds. She was always highly strung and excitable and whenever she got the slightest thing wrong with her, she always thought she was going to die. She was a great believer in patent medicines, and took much self-prescribed medication. There was a history of a mental illness in 1945, shortly after her husband was drafted overseas. She became very depressed, lay in bed all day, neglected her appearance, became careless and untidy in the house. She lived alone in a flat with her little girl at this time and believed the neighbours were against her and talked about her. She lived in constant fear and persistently believed that she was followed by men. She was awkward, unpredictable and difficult to deal with. She was actively hallucinated and would often scream out because she saw a man in her room. The illness lasted 2-3 months and she recovered spontaneously two weeks after going to live with relatives. No admission to hospital was necessary on this occasion. The family are convinced that the cause of this breakdown was anxiety over the absence of her husband.

DISCUSSION

Fischer (1953) has suggested that the schizophrenic might have a genetically determined abnormally low threshold for stress. In this case there were two well-authenticated breakdowns, the first one apparently in response to a psychological stress, the second undoubtedly as a result of an infection. Gellhorn (1953) and Hoskins (1946) have attempted to describe schizophrenia in physiological terms, and Gerard (1955) postulates an inherited biochemical aberration as a dominant factor in the causation of schizophrenia.

Gjessing (1953) has repeatedly claimed that a disturbance of nitrogen metabolism in a fluctuating fashion accompanies the behavioural changes in episodic catatonia, and using neurosurgical material, Scharenberg and Brown (1954) describe amoeboid degeneration in astroglia from catatonics. They compare the appearances to similar changes brought about by severe disorders of metabolism—one of which is infectious disease. This seems to them to suggest a profound disturbance of metabolism in catatonic states. Yet the attempt to demonstrate a catatonigenic agent remains disappointing and recent workers have had to refute earlier successful claims, e.g. Edison (1956) using blood serum and urine of catatonic patients and Shapiro (1956) using cerebrospinal fluid have alike reported negative results.

The response to amytal is of course not new (Solomon, Kaufman and D'Elseaux, 1931; Smith and Schwartz, 1934; Thorne, 1938; Elkes, Elkes and Bradley, 1954), though it has more commonly been reported in chronic than acute states. The behavioural change was all the more striking here therefore and is taken as strong evidence that the diagnosis of catatonic schizophrenia was beyond doubt.

The EEGs confirm the clinical impression that catatonic stupor is a state somewhat between sleeping and waking (Mayer-Gross, Slater and Roth, 1954). The records were shown independently and without clinical history to two experienced electroencephalographers (J.W.O. and A.S.) and both agreed that sleep was the most likely explanation of the paroxysmal slow activity. Colony and Willis (1956) in a recent report of the EEGs of 1,000 schizophrenics found only three examples of activity at $1\frac{1}{2}$ –3 c.p.s. one of which was a severe acute catatonia. Their material was unusually valuable as it included 822 acute cases. From their results and a good review of the literature they conclude that EEG abnormality is rare in schizophrenia, possibly 5 per cent., and they suggest that schizophrenia is therefore not an organic disease. "Some of the people who become schizophrenic under some disintegrating psychological threat may have

had previously abnormal EEG patterns and the EEG abnormality noted as part of the clinical picture of the schizophrenia was either there to begin with, i.e. was coincidental, or was developed after the inception of schizophrenia as a result of either sequelae or intercurrent factors." It is very unlikely, they say, that EEG changes which are not static and which may change even in the course of the same examination are adequate evidence to support a genetically oriented or organic concept of schizophrenia. They see their results as supporting the view that it is a functional disease of psychological origin.

CONCLUSION

A case is presented showing the onset of acute catatonia as a sequela of middle lobe pneumonia. The history also shows a schizophrenic breakdown on a previous occasion apparently due to psychological stress. The characteristic response of catatonic schizophrenia to intravenous amytal is once again demonstrated. The state of active negativism is accompanied by changes in the EEG resembling sleep though the subject was "awake". Although the patient was dangerously ill from her pneumonia and for this reason E.C.T. was not given, yet in the end a fatal outcome occurred. It would seem therefore that in such cases, no matter how desperately ill the patient appears to be, it is probably not justifiable to withhold E.C.T. which often specifically terminates the state. "The hyper-acute catatonic patient with confusion, fever, and leucocytosis may very well die under one's hand . . . and three convulsion treatments on successive days or even more as a life saving measure may produce dramatic improvement" (Sargant and Slater, 1955).

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REFERENCES

- COLONY, H. S., and WILLIS, S. E., "EEG studies of 1,000 schizophrenic cases", *Amer. J. Psychiat.*, 1956, **113**, 163-169.
- EDISON, C. B., "Studies of the toxicity of schizophrenic blood serum", *Dis. Nerv. Syst.*, 1956, **17**, 77-80.
- ELKES, J., ELKES, C., and BRADLEY, P. B., "The effect of some drugs on the electrical activity of the brain, and on behaviour", *J. Ment. Sci.*, 1954, **100**, 125-128.
- FISCHER, R., "Stress and the toxicity of schizophrenic serum", *Science*, 1953, **118**, 409-410.
- GELLHORN, E., *Physiological Foundations of Neurology and Psychiatry*, 1953. St. Paul: The North Central Publishing Co.
- GEORGI, F., RIEDER, H. P., and WEBER, R., Remarks on Fischer's article "Stress and toxicity of schizophrenic serum", *Science*, 1954, **120**, 504-506.
- GJESSING, R., *Arch. Psychiat. Nervenkrankh.*, 1953, **191**, 191.
- GERARD, R. W., "The biological roots of psychiatry", *Amer. J. Psychiat.*, 1955, **112**, 83.
- GOULD, J., "The use of vitamins in psychiatric practice", *Proc. Royal Soc. Med.*, 1954, **47**, 3, 215-219.
- HOSKINS, R. G., *The Biology of Schizophrenia*, 1946. New York: W. W. Norton.
- LOCHER, R., "On the sudden death of mental patients and the acute catatoniform syndrome with fatal outcome", *Msehr. Psychiat. Neurol.*, 1941, **103**, 278.
- MAYER-GROSS, SLATER, E., and ROTH, M., *Clinical Psychiatry*, 1954. London: Cassell & Co.
- RIEDER, H. P., "Biological determination of toxicity in pathological body fluids", *Confinia Neurologica*, 1954, **14**, 65-87.
- SARGANT, W., and SLATER, E., *Physical Methods of Treatment in Psychiatry*, 1954. London and Edinburgh: E. & S. Livingstone.

- SCHARENBERG, K., and BROWN, E. O., "Histopathology of catatonic states", *J. Neuropath. Ex. Neurol.*, 1954, **13**, 592-600.
- SHAPIRO, A. K., "Attempt to demonstrate a catatonigenic agent in the cerebrospinal fluid of catatonic schizophrenic patients", *J. Nerv. Ment. Dis.*, 1956, **123**, 1, 65-71.
- SMITH, P. S., and SCHWARTZ, D. K., "Sodium amytal as a means of obtaining contact in stuporose and uncommunicative cases", *Psychiat. Quart.*, 1934, **8**, 748.
- SOLOMON, H. C., KAUFMAN, M. P., and D'ELSEAUX, F., "Some effects of the inhalation of oxygen and carbon dioxide and of intravenous sodium amytal in certain neurological conditions", *Amer. J. Psychiat.*, 1931, **10**, 761.
- THORNE, M. W., "Psychologic structure of catatonia—a psychopharmacological survey using sodium amytal", *Arch. Neurol. Psychiat.*, 1938, **39**, 513-517.

DIMETHYLTRYPTAMINE EXPERIMENTS WITH PSYCHOTICS

By

Z. BÖSZÖRMÉNYI, M.D., Ph.D.

and

ST. SZÁRA, M.D., Ph.D.

From the Central State Institute of Nervous and Mental Diseases, Budapest

THE so-called experimental psychoses have been in the focus of increased interest recently, as a result of the discovery of new hallucinogenic or psychotic agents and of the fact that the problems can be approached more closely even in the borderline fields of pathopsychological research (biochemistry, electrophysiology, etc.) owing to advance in technique. Thus, it appears that Kraepelin's dream of producing so-called miniature psychoses by the administration of substances foreign to the body will come true after all.

In the research on experimental psychoses the trend has now shifted from the observations made on normal subjects to trials for the eventual usefulness of these agents in therapy, as is proved by the data reported in the literature for mescaline and LSD. There are two trends of thought in the therapeutic use of these agents. The two extremes are represented by the Delay school and by Sandison and associates (1954). The former want to use mescaline essentially for inducing a vegetative shock effect, whereas the latter want to utilize LSD as an aid in psychotherapy. Delay *et al.* (1956) have emphasized also the significance of mescaline in diagnosis and prognosis.

These two best-known psychotic agents have been joined recently by a new group, that of the tryptamine derivatives, of which two members: bufotenine and dimethyltryptamine (DMT) have been described in the literature. These two indolamines are in close structural relationship to a biogen-amine, serotonin, which has been arousing interest recently, and which is chemically 5-hydroxytryptamine. Bufotenine is the dimethyl derivative of serotonin, whereas DMT differs from the former in that it contains no 5-OH group. Fabing (1956) was the first to study the effect of bufotenine on man and claimed that bufotenine widely occurred in nature, its main source being the fungus *Amanita*.

Our team (Böszörményi, Brunecker, Sai-Halász and Szára) studied for the first time the effect of DMT on man. The drug was synthesized by Szára (1956), a biochemist, who also described the metabolic data. As it had been found that the drug, as administered per os, caused changes in mood and a mild disturbance of perception in a few subjects only, intramuscular injections were given. The experimental subjects were 30 normal individuals, most of them doctors. Double protocols were kept and subjective experiences were recorded later. The experimental psychosis elicited by 0.7 to 1.0 mg./Kg. body weight of DMT begins very rapidly (within 3 to 5 minutes) and is over in about 50 to 70 minutes. This model psychosis is characterized by very intense vegetative phenomena, anxiety, and in some individuals by the neurological signs suggesting a temporary impairment in the function of the so-called non-

dominant hemisphere, by disturbances in mood and perception, first of all by visual illusions. Alterations in the perception of space and time, loosened associations and in some cases mild ideas of reference also occur. In many of its features this disturbance was similar to the single phases of the LSD effect, but bore no definite resemblance to either the schizophrenic or the other endogenous-psychotic clinical states.

In the following we wish to discuss the experiments we have made on patients with mental diseases. The test subjects were 24 female in-patients at our department, each of whom was given a single dose of 1 mg./Kg. of the drug i.m. In 3 cases the experiment has been repeated with a dose of 1.5 mg./Kg. The majority of the subjects suffered from chronic schizophrenia for from 3 to 20 years (20 cases). The other patients involved had oligophrenia (2 cases), psychopathy (1 case), and conversion hysteria (1 case). To illustrate the experiments, 3 protocols are described.

Case 1. K.V., aged 30 years. Premorbidly she had been sensitive, reserved. Three years before admission she developed hyperthyrosis, loss of weight. Since then she has become more and more unsociable, suspicious and made unfounded accusations against her colleagues.

When first admitted in 1955 sensitive ideas of reference connected with an earlier love affair, overestimations, persecutory delusions and paranoid behaviour were found. Electric shock (E.C.T.) and Largactil treatment resulted in a fading of pathological contents, in a more sociable attitude and she was discharged in an improved condition.

In 1956 she relapsed, developing very extensive, partly systematized paranoid ideas and most unsociable behaviour. E.C.T., insulin and Largactil therapy resulted in improvement.

30 April, 1956. DMT Experiment (1 mg./Kg. intramuscularly)

Following injection at:

- 6 minutes She complains of a strange feeling, tinnitus, buzzing in the ear. The eyebrows show tic-like contractions.
- 10 minutes She is smiling silently. When asked why she is smiling she is unable to tell. She still complains of feeling herself inexplicably strange.
- 15 minutes She wants to get up, but is unable to do so. Looks around helplessly, turns about in the recumbent position.
The pupils are maximally dilated; she complains of dyspnoea and blurred vision.
- 20 minutes Nausea and numbness of the left hand are complained of. She feels her whole body rigid, as if she could not move. Blood pressure sinks from 130 mm. Hg to 110 mm. Hg. Pulse rate is unchanged (80/min.).
- 23 minutes She tells us laughing that her dyspnoea and nausea have become more intense. She keeps asking: "Why do I feel so strange?"
- 25 minutes Complaints unchanged, she looks about vaguely.
- 30 minutes Vision is improving, but everything seems to be so strange still. There are buzzing in the ears and numbness of the limbs, particularly in the left hand.
- 32 minutes "As if I had no heart and my head, too, feels so strange, I don't know what this is, but I feel as if I were not myself."
- 35 minutes She feels herself exaggeratedly light, a rather unpleasant feeling. Extremitals numbness has diminished.
- 38 minutes "I don't like this feeling, as if my head were beginning to clear up. I saw such strange dreams, but at the beginning only . . . I saw strange creatures, dwarfs or something, they were black and moved about . . . Now I feel as if I were not alive . . . as if I were floating between earth and sky . . ." She feels as if her limbs were not her own.
- 41 minutes Her head is clear now, she feels definitely "relaxed". "As if I were not I, such a light feeling." The left hand is still numb.
- 45 minutes She has strange feelings in the internal organs only. "As if my heart would not beat, as if I had no body, no nothing, all I feel are my left hand and stomach."
- 50 minutes The dilatation of the pupils has ceased. She feels her head "clear". She tells us she feels herself too quiet, an unusual experience, "I don't like to be without thoughts" she has never felt like this before. She tells us that at the beginning she felt as if she had been directed by someone else, things happened not according to her will, when she moved it was as if it had happened against her will.

Afterwards, she thought the experiment to have been an interesting treatment, and did not ask about its purpose. She did not include it into the pattern of paranoid ideas. She kept repeating for long (over months after the experiment) that she was "unusually quiet, as if her head were quite empty". Earlier thoughts lost some of their intensity. After DMT the psychotherapeutic contact with her doctor has considerably improved.

The reaction of this patient, a case apparently one of sensitive paranoid borderline, stood nearest to that of the normal test subjects; during experiment depersonalization and influential ideas also appeared.

After a period of improvement the patient had to be re-admitted because of typical diffuse paranoid manifestations.

Case 2. Mrs. I.P., aged 25 years. Before disease she had been unsociable, primitive, day-dreaming. She had lost her mother early and had been educated for 10 years in a cloister. She married at the age of 16 years. Although she felt attached to her husband, marriage meant for her a complete seclusion from the world and from independence. After the first complicated parturition she began to feel attracted by a distant male acquaintance and day-dreamed about him. After the second gestation she developed extreme anxiety, self-accusatory ideas, obsessions and hallucinations. She was admitted to our department in 1953 and improved on E.C.T. and Insulin therapy. She was re-admitted in 1954, after one year at her home. When re-admitted, marked "Wahnstimmung", ideas of omnipotence, uncertainty of ego, obsessional qualities were found. While at our department her condition was characterized by self-accusatory ideas associated with extreme anxiety (the world will perish because of her guilt) changing into ecstatic episodes of scenic-wish-fulfilling nature, with sexual hallucinations. The repeated E.C.T. and insulin treatments brought temporary improvement only.

11 October, 1956. DMT Experiment (1 mg./Kg. intramuscularly)

Blood pressure: 140 mm. Hg systolic, 90 mm. Hg diastolic. Pulse rate: 76/min.

Following injection at:

- 8 minutes Her mood heightens, the pupils dilate and the associations are loose.
- 10 minutes She is silent, does not answer questions. From time to time she places her hand on her forehead, moving the head. "Oh, now", moves her finger menacingly.
- 13 minutes Blood pressure is unchanged (140/90 mm. Hg). Pulse rate: 80/min. She does not answer questions, is lying silently, smiling happily. At intervals she whispers almost inaudibly: "Oh, sure . . . then I . . . that is . . . only people should not . . ."
- 15 minutes Moves her head right and left, making directing motions with the index finger. She laughs at times: "Oh, sure . . ." Babinski's sign could be elicited weakly on the right side.
- 18 minutes She is scrutinizing her environment. "People seem to be more serious now." She seems to identify the doctors with old friends. Makes uncertain gestures with the hands.
- 20 minutes She keeps looking at her left hand, playing with her fingers. "Even the sun has disappeared . . . I had to produce . . ." "Heat from cold, or reversed? But let us not hurt each other, let us not take things so seriously."
- 23 minutes "So many things have happened . . . it is true that I . . . I do not wish wrong to anyone . . ." She laughs, becomes silent again, makes stereotyped waving movements with her left hand, does not answer questions.
- 26 minutes She is looking around and can be slowly contacted. She speaks incoherently, with increasing intensity about her earlier experiences at home and in the hospitals, mentioning her children.
- 30 minutes She is looking out of the window, with an ecstatic smile on her face: "How beautiful? How can this be now? . . . Who have done it? How? . . . All feeling and happiness have been accumulated and filled into me or what . . . what has been left for people? . . ." She notices any small things and finds delight in them, emphasizing that she does not deserve such happiness and does not want to cause sorrow to anyone.
- 35 minutes The drive to speak lessens, she keeps putting her hands to her head. "I can't understand how they have done it . . ." Her eyes are full of tears for pleasure and delight.
- 40 minutes The state of ecstasy is over. The pupils are of moderate diameter. She is quiet, slightly vague and suspicious.

Two days after experiment she became agitated, quarrelling and aggressive with her husband and doctors.

(In the mainly hebephrenic patient characterized by some paranoid features the DMT effect caused an illusionistic change in the environment and a short period of ecstasy, similar to, but more intense than what has occurred spontaneously.)

Case 3. Mrs. I.M., aged 38 years. Premorbidly she had been quiet, of jolly mood, living a happy married life. In 1954 her husband was murdered and soon afterwards her only child, too, died. Her second marriage was unhappy, her husband being untrue to her. One of her stepsons often ran away from home and went stealing.

A girl friend of her husband, of whom she is jealous, gave her a small volume of wine, after the drinking of which she developed symptoms of tenebrosity and had to be brought to a mental hospital in 1956. She asked to be put on a tapering-off cure, in the course of which she developed tenebrosity with visual hallucinations and agitation on the ingestion of the smallest doses of alcohol; in every case the contents were those causing her conflict.

She had been re-admitted in the same year, in a state of tenebrosity after attempting suicide with barbiturate. On re-admission hallucinations closely in contact with her conflict material and agitation characterized her condition. She cleared up after having been given Largactil for a few days.

15 October, 1956. 1 mg./Kg. of DMT is administered intramuscularly

Blood pressure: 150 mm. Hg systolic, 90 mm. Hg diastolic. Pulse rate: 84/min.

Following injection at:

- 5 minutes She complains of numbness in the limbs. "I don't know what is happening to me." The pupils are dilated. She is making vague movements with her left hand, holding her head. She looks scared, "My vision is so blurred".
Blood pressure: 180/100 mm. Hg. Pulse rate: 100/min.
- 10 minutes Her lips are trembling, she sighs deeply, plays with the fingers of her left hand, turning her head. She answers questions slowly. She sees her hands coloured.
- 13 minutes A number of people enter the room. She looks up scared, "Why do these people walk about here?". We cover her with a blanket, which she feels with her hands then throws away, "Do not put my husband's cerement over me . . . (looking scared at the ceiling) look, what is there . . . a hanged man . . . oh, cover him with something, I am so scared . . ."
- 20 minutes She hears noise and talk from the outside. Policemen have come, as she thinks to get her stepson who had run away from home.
- 25 minutes She continues to talk with the policemen, abuses the girl friend of her husband and menaces her husband. She is walking about agitatedly, touching the furniture. Does not answer questions, sighing deeply, hands trembling. At a table covered by a bed sheet she begins to cry, "Someone has died there, oh my child . . . My God . . . a beautiful cerement, my son's . . ." She looks at the coat-stand with terror in her eyes: "Take off that poor man, he had been hanged for killing my husband . . ." She is still tenebrose and agitated two hours after injection. We give 50 mg. of Largactil, on which she slowly becomes quiet.

(Thus, in this case mild visual hallucinations were followed by the development of a hysterical episode, with scenic hallucinations based on her conflict material.)

We have presented the first case mainly because her response to DMT was the most similar among the schizophrenics to the model-psychosis of normal subjects. The second patient showed the most colourful response in the group, experiencing a change in her environment. In the third case the DMT effect was almost completely overmasked by the tenebrosity, similar to what has taken place repeatedly under the influence of alcohol.

The symptoms shown by the 24 test subjects during the DMT effect may be summarized in brief as follows:

Vegetative Disturbances

These began 6 to 12 minutes after injection, with a dilatation of the pupils accompanied by a rise in blood pressure that was marked (30 to 40 mm. Hg) in 3 cases, and moderate (10 to 15 mm. Hg) in 12 cases. Pulse rate increased to 96, 100/min. Blood pressure sank moderately in 5 patients. Tachypnoea, sighing and yawning were noted in a few cases. These symptoms were accompanied by subjective complaints in about 50 per cent. of the cases, as revealed spontaneously or on questioning. The negative change in their condition the patients usually denoted by saying they were feeling "strange". A smaller percentage of patients complained of anxiety, agitation, protested against the "treatment", whereas another group behaved in a seclusive manner.

In 4 patients with chronic schizophrenia 1 mg./Kg. of DMT caused neither vegetative symptoms nor any subjective complaints, the behaviour remaining absolutely unchanged during experiment. In the 3 patients given 1.5 mg./Kg. the symptoms were more intense, accompanied by vomiting in 2 cases.

Psychomotor Disturbances

Unmotivated laughing was seen in 11 cases, of whom 4 pointed out its strangeness.

"G.J.: Why do you hypnotize me, to laugh at me, what do you want, please, there is nothing funny here . . ."

"G.P.: Now they are doing things to me again . . . they exchange me . . . I have been laughing a short while ago . . . it was not me . . . but the other women I was exchanged for . . ."

Anankastic-like stereotyped motions, such as waving of arms, rhythmic grasping motions, pulling at the clothes, etc. were observable in 8 cases. These motions were discontinued when we asked about them; the patients denied having carried them out, declaring that they did not want to move, they "had" to move. In one hebephrenic the increased motor activity increased to jactitation, accompanied by an ecstatic experience of sexual nature. The patient made embracing motions with the arms and legs, opening her mouth, biting at her clothes and hands. In another patient a stupor-like condition developed during experiment and the only motor activity was the sucking motions in the lip muscles. In the three experiments in which the higher doses were employed intense motor activity developed, with athetoid and partly ballistic motions.

Neurological Symptoms

Owing to the poor co-operation, it was very difficult to examine the patients neurologically. In one patient a positive Babinski's sign could be elicited for about $\frac{1}{2}$ hour on the right side. Ten patients complained of paraesthesia. In most cases disturbances of co-ordination, manifested mainly in an ataxic quality of walking, were also observable.

Changes in Mood

These occurred in the majority of the test subjects. Euphoria was marked in four cases, of whom in the three hebephrenics it was associated with giggling and in two with a feeling of happiness. In further five patients the heightening of mood was manifested mainly in an increased drive to speak and in liveliness. Three patients have become definitely depressed: they were crying, speaking about earlier offences, feeling themselves to be failures. One patient expressed a wish to die, which she has never done before.

Disturbances of Perception

An illusionistic change in the environment, hallucinatory experiences have been spoken about but by a few patients. Two oligophrenics and one psychopathic patient told us about lively visual hallucinations spontaneously, as occurring with the eyes either open or closed.

H.M.: "How funny . . . I see such nice colourful things . . . and everything is moving . . . there are all kinds of figures and your face, too, is so distorted and coloured . . ."

F.M.: "My hands are so beautifully coloured . . . such ornate leaves or whatnot are on the wall . . ." . . . "As if the door had moved, but no . . . even the wall appears to be undulant."

Only one of the schizophrenics saw vague shapes. Acoustic hallucinations have not occurred. Two schizophrenics, who had intense acoustic hallucinations before experiment, were seen to have them greatly increased during experiment, with marked anxiety and agitation.

G.I.: "Oh, what they are telling me . . . why do they make faces at me . . . do not say so! Oh, my God . . . Please, maybe I am not insane at all . . . please, tell them these dirty things."

Thought Disorder

In the overwhelming majority of our schizophrenics typical thought disorders existed. During the DMT experiment these increased in some degree quantitatively, with an increased incoherence and a more frequent occurrence of evanescence of thoughts. We have not observed that any pathological productions new in content would have been brought to the surface.

Personality and DMT Effect

It has been found that pre-existent pathological changes in personality have become more plastic during DMT intoxication. This applies first of all to the schizophrenics. As the DMT effect is over in a relatively short time, within 30 to 50 minutes, no more detailed explorations could be carried out, all that could be observed being a typical situative behaviour. In general, every patient was aware of the correlation between the injection and the change she was experiencing. Apart from this, however, they evaluated the situation very differently. Most of the paranoid schizophrenics assumed a hostile attitude toward the treatment, some of them accusing either the doctor or the persons involved in their persecutory ideas of poisoning them. Some patients seemed to care little or not at all with the experience of their life being in danger, whereas others quarrelled and protested actively. Only a few co-operative patients showing good contact with their doctors even before experiment showed evidence of asking for help and of an improvement in contact.

Emotional reactions were seen to develop either to the immediate situation, or to pre-existing pathological ideas. A hebephrenic patient who had been concerned persistently with sexual ideas and voiced erotomantic ideas spoke of ecstatic happiness during the experiment, attributed sexual qualities to everything in her environment, irrespective of sex or object. Another hebephrenic kept repeating her pre-existing erotomantic contents: "You want to marry me, don't you?" she kept asking the experimenter. Four schizophrenics have become definitely autistic during the experiment. A complete stupor was seen in one case. After it ceased, the patient stated to remember nothing, "I must have been unwell". These patients had already exhibited autistic features before the experiment.

DISCUSSION

DMT acts apparently on the area of the meso-diencephalon, first of all on the vegetative centres of the hypothalamus and on the thalamus, and secondarily on the extra-pyramidal nuclei and, less intensely, on certain cortical areas. It may be assumed that here, too, a stimulation of the meso-diencephalic-activating system plays a role, just as it has been substantiated by Himwich (1956) for LSD on the basis of animal experiments. Just like the states produced by mescaline and LSD, the model psychosis elicited by DMT corresponds to one form of the exogenic reaction type of Bonhoeffer. In this the conspicuous feature is what Meggendorfer (1928) stressed for mental disorders due to intoxication, notably that the mental disorder is the result of an interaction between the poison and the poisoned organism. Individual differences are obvious in its manifestations and the differences are the result of an interaction between the continuously varying living conditions and hereditary factors. Even though the DMT psychosis is hyperacute in course, individual differences are obvious, especially with normal experimental subjects: those showing a tendency to anxiety can be differentiated from the mild paranoid querulent types, from

the well-balanced meditators, etc. In some cases a conspicuous parallelism could be observed between the leading and view of life, on the one hand, and the DMT experience, on the other. Even in the normal test subjects an accentuation of the personality qualities occurred, alongside the loosened consciousness. Subconscious material which had been repressed or held secret did not upsurge in any of the normal test subjects; of course, in this relation great significance is to be attributed to the fact that most of the normal test subjects were doctors (21 out of 30), who have been more or less conscious of the experimental situation throughout the experiment. But even with the non-doctor test-subjects only a very few too-individual manifestations occurred, even when the most dramatic vegetative changes were taking place. Thus, on this basis DMT appears to be unsuitable for use in explorative studies on neurotics.

The question arises whether DMT is suitable for use as an aid in self-experiments of young psychiatrists, or for auto-analysis, or for trying to approach the mental processes of diseased persons, as LSD has been recommended by Felsing *et al.* When the liver and circulation are normal, when DMT is administered carefully in doses not exceeding 0.9 mg./Kg. there is apparently no danger resulting from its use. Although the mild disturbance of liver function which has occurred afterwards in two test subjects has disappeared within one week, we do not think DMT to be suitable for use. For, as has been seen, the psychic disturbance of suitable degree is accompanied by a very strong sense of ill-being which makes concentration and realization of the experience rather difficult. Persons with marked eidetic qualities may be exempt from this rule; these may respond to smaller doses which do not cause anxiety or such stress effects as may completely overmask the specific effect of the drug. Also the too rapid course of DMT psychosis makes the drug unsuitable for use for this purpose. Thus, in this respect DMT cannot compete with mescaline and LSD.

The DMT effect is similar to the LSD effect in that only illusions develop in most test subjects. Likewise, the heboïd behaviour, the partly empty euphoria, the forced laughing, the weakness of concentration, the loosened and divertible associations, the over-estimation of time, etc. are apparently common features. On the other hand, the frequent occurrence of paranoid reactions resembled the mescaline effect, though these did not reach the degree of references observable in mescaline intoxication; also during the DMT experiment only a relatively few spontaneous paranoid manifestations have occurred, most of the patients mentioning them only afterwards.

The following differences in DMT psychosis between the normal and psychotic test subjects should be pointed out:

(a) In schizophrenics the vegetative symptoms developed by an average of 2 to 4 minutes later than, and in the non-schizophrenics at about the same time as in the normal test subjects. In view of the fulminating course of the DMT effect even such a small time lag is significant.

(b) In general, the vegetative symptoms were less intense in schizophrenics, four of whom showed no vegetative symptoms at all. In these patients no change in behaviour could be noted and apparently no subjective experiences characteristic for the DMT effect had taken place.

(c) Except in a sensitive borderline case, DMT has produced no visual hallucinations in schizophrenics, although such hallucinations occurred as a rule in the four non-schizophrenics. In the three cases in which the experiment has been repeated with the 1.5 mg./Kg. dose no changes suggesting hallucin-

ations could be observed in the presence of marked vegetative manifestations, anxiety and severe psychomotor agitation.

(d) In the patients with schizophrenia no late effects causing changes in mood or behaviour have been noted, nor did such patients relate such subjective experiences afterwards, except for K.V., whose case history was presented first and whose case could be considered to be in part one of sensitive reference.

In both groups DMT accentuated pre-existing pathological features of personality, although in the normal test subjects this could be held under control in some measure by self-criticism. The disturbance of perception placed a greater burden on the normal test subjects than on the psychotics, who, if they had such disturbances at all, accepted them more readily, having been "accustomed" to them by the pre-existent changes in personality.

Just like LSD, DMT causes a severe stress in the normal subjects, who respond to the drug with anxiety and restitutive activity. This secondary response often exceeds the primary one; this feature has been less marked in the cases of schizophrenia, at least in most of them.

The delayed onset and the lesser intensity of vegetative symptoms, the absence of disturbances in perception and of late effects make us agree with Condrau (1949), who asked whether schizophrenia itself may not be the cause of the diminished sensitivity to LSD, or in our case to DMT. This could be brought into correlation with the problem of the pathogenesis of schizophrenia, a problem as old as the conception of the disease itself. In 1896, Kraepelin outlined the symptoms and course of dementia praecox, suggesting an "auto-intoxication" of endocrine nature as the cause. In up-to-date wording this theory would sound like this: schizophrenia is due to a disturbance of the enzymic systems influencing the cerebral synaptic connections. This disturbance may arise as a result of an action by hallucinogenic agents of animal origin (Fabing, 1956); tryptamine, bufotenin and adrenochrome may be the chemical basis of the single schizophrenic syndromes. According to Fabing the new ataraxics (reserpine, chlorpromazine, pipradrol) may exert a favourable action by counteracting some of the metabolic anomalies in schizophrenia, although these agents differ in chemical structure from the indol bases mentioned above. Unfortunately, the role of serotonin in normal cerebral function has not been elucidated and for this reason will not be discussed here.

Nevertheless, on grounds of what has been elaborated above it seems justified to assume that in a certain percentage of schizophrenics metabolites similar to DMT may be formed as a result of a primary or secondary impairment of tryptophane metabolism and may in part be responsible for the pathological symptoms. This would explain the less intense response to DMT. Recent biochemical evidence shows that in schizophrenia the blood contains significantly more ceruloplasmine (an enzyme containing copper) than in normal subjects (Akerfeldt, 1957; Abood *et al.*, 1957), and this, too, may account for the faster rate of DMT breakdown. Another possibility is that in the schizophrenic some receptors are already occupied by endogenous endotoxins and thus the psychotic agent can produce no further symptoms; this may be the explanation for the absence of new hallucinations after the administration of DMT.

The data heretofore obtained do not permit us to draw final conclusions as to the use of DMT in the treatment of mental disease. Although a temporary improvement has resulted in three cases, it is very difficult to tell whether this could be ascribed to the effect of DMT or to the increased attention devoted to the patient during experimentation. Unfortunately, the majority of the patients

could be given but a single dose of the drug, except for the three patients tested with DMT twice. Thus, our observations cannot be compared to those made in connection with the therapeutic effect of LSD, as far as precision is concerned. No deterioration ascribable to the DMT effect had been noted in our cases. We are planning to study schizophrenics who will be given smaller doses of DMT over prolonged periods of time. Nevertheless, DMT seems to be more suitable for use as a diagnostic and prognostic aid in the differentiation of the different schizophrenic defect states, as it has been found that the less numerous were the symptoms, i.e. the more "burnt out" was the patient, the response to DMT was the poorer. This, however, cannot be stated with certainty until further observations have been made.

On grounds of the evidence obtained in the trials with DMT we have drawn the conclusion that every psychotic drug should be tried out in psychotics also, so as to enable us to obtain a more plastic picture of its effects. Although in psychoses the personality is already distorted in some measure and some of the responses are less marked, it is usually possible to make a more detailed and frank analysis of the pre-morbid and pathological personality than with the normal test subjects. Moreover, to use Marti-Ibanez's phrase (1956), the psychotic agents, as chemical keys, may help to open certain doors through which we may gain insight into the symptoms or even into the pathomechanism of psychosis. Thus, the additional strain represented by the poisoning may magnify latent subclinical anomalies and thus may open them up for observation. It is the task of the future to decide which psychotic agent is the most suitable for this purpose.

SUMMARY

The authors have tested the response of 24 female patients to experiments with dimethyltryptamine, using 1 mg./Kg. body weight single doses. The majority of the patients (20) were chronic schizophrenics.

After presenting an analysis of 3 illustrative protocols the differences in response between normal test subjects and psychotics are discussed.

Importance is attributed to the fact that in schizophrenics the onset of vegetative symptoms was delayed, the vegetative manifestations were less intense and were even absent in 4 cases.

The drug produced no new hallucinations. After-effects have not occurred.

The reaction given by the non-schizophrenic psychotics was similar in course to that given by normal test subjects.

The reduced sensitivity to dimethyltryptamine observed in schizophrenics suggests the possibility that a disturbance of cerebral metabolism is in the background of the psychic process of schizophrenia, as has been postulated by many authors in connection with LSD experiments.

It is believed that the drug may prove to be a useful aid in the appraisal of prognosis in schizophrenia.

REFERENCES

- ABOOD, L. G., *et al.*, *Arch. Neurol. Psychiat.*, 1957, 77, 325.
AKERFELDT, S., *Science*, 1957, 125, 117.
CONDRAU, G., *Acta Psych. et Neurol. Scand.*, 1949, 24, 9.
DELAY, J., *et al.*, *Ann. Méd.-Psychol.*, 1956, 114, 292.
Idem, ibid., 1956, 114, 300.
FABING, H. D., *Amer. J. Psychiat.*, 1956, 113, 409.
Idem, *Ref.: Excerpta Med. VIII*, 1956, 9, 365.
V. FELSINGER, J. M., *et al.*, *J. Clin. exper. Psychopath.*, 1956, 17, 414.
HIMWICH, H. E., *Dis. Nerv. Syst.*, 1956, 17, 109.
MEGGENDORFER, F., *Intoxicationspsychosen. Handb. d. Geisteskrankheiten*. Bd. VII, 1928. Berlin, Springer.
SANDISON, R. A., *et al.*, *J. Ment Sci.*, 1954, 100, 491.
SAI-HALÁSZ, A., BRUNCKER, Gy., and SZÁRA, St. To be published.
SZÁRA, St., *Experientia*, 1956, 12, 441.
MARTI-IBANEZ, F., *J. Clin. exper. Psychopath.*, 1956, 17, 360.

A CONTROLLED CLINICAL TRIAL OF MEPROBAMATE IN THE MANAGEMENT OF DIFFICULT AND DESTRUCTIVE FEMALE MENTAL DEFECTIVES

By

W. A. HEATON-WARD, M.B., Ch.B., D.P.M.

*Medical Superintendent
Stoke Park Hospital Group, Bristol*

and

J. JANCAR, M.B., B.Ch., B.A.O., D.P.M.

*Senior Hospital Medical Officer
Stoke Park Hospital, Bristol*

MEPROBAMATE is the shortened name of 2-Methyl-2-n-Propyl-1,3 propanediol dicarbamate, which is also known as Miltown or Equanil.

Previously published reports (1-5) on clinical trials of this drug have concerned its effect on patients suffering from neurotic and psychotic disorders. As far as we are aware no reports have been published of its effects on mentally defective patients.

The clinical trial was part of a long-continued search for a drug to control the behaviour disorders of mental defectives without affecting their level of consciousness. The behaviour of the patients chosen for inclusion had failed in each case to respond over the years to a large range of sedative drugs including, most recently, nine cases who had failed to respond to Largactil (chlorpromazine hydrochloride) and four cases who had failed to respond to Rauwiloid (*Rauwolfia serpentina*).

SELECTION OF PATIENTS

The sixteen patients selected for this trial all had long histories of mental instability manifesting itself in overt aggression to staff and other patients and in destructiveness of clothes and other property. They were aged between 20-54 (all but two were between 20-35) with mental ages below 6 years. No patient was included who showed evidence of any specific super-imposed psychosis, but six patients suffering from epilepsy were included.

The patients were allocated alternately to the meprobamate and control groups. This resulted in four of the epileptic patients being in the former and two epileptics in the latter group. One patient allocated to the control group developed an intercurrent infection before the trial began and was excluded. One patient in the control group developed cellulitis and was excluded after four weeks and one in the meprobamate group deteriorated mentally and was excluded after eight weeks.

PRELIMINARY INVESTIGATIONS

All patients were weighed before the beginning of the trial and at weekly intervals. The urine and blood were examined before the beginning of the trial

and at monthly intervals. The blood pressure was measured at first weekly and later monthly. An EEG was performed on all patients except one, who was too unco-operative, before the trial was begun and repeated on the patients receiving meprobamate during its last week. A four-hourly temperature, pulse and respiration chart was kept for each patient.

PROGRESS REPORTS

In an attempt to make the reports of the nursing staff as objective as possible, we devised a questionnaire in the form indicated below, which required a numerical answer to each question. Thus, deterioration or improvement in any respect was indicated as a higher or lower numerical answer to each question. The nursing staff completed this questionnaire for each patient at weekly intervals throughout the trial.

1. How many times in the past week has he/she attacked staff or other patients?
2. How many times in the past week has he/she been in seclusion?
3. In the past week how many articles of clothing has he/she deliberately damaged?
4. In the past week how many other articles has he/she deliberately damaged?
5. In the past week on how many occasions has he/she been insolent towards staff?
6. In the past week on how many occasions has he/she been noisy and excitable apart from those reported under 1-5 above:
 - (a) during the day?
 - (b) during the hours of sleep?

DOSAGE

The meprobamate tablets (400 mg.) were known to nursing staff only as tablets number 6 and the control tablets only as tablets number 5.

Patients in each group were given a commencing dosage of one tablet t.d.s. After four weeks this was increased to 5 tablets per day in divided doses and a week later to 2 tablets t.d.s. The trial was continued for seventeen weeks from its commencement.

RESULTS

We were unable to observe any improvement in the behaviour of the patients in either group and in no case did the answers to the questionnaire reveal any sustained numerical reduction which appeared to be significant.

PHYSICAL CHANGES DURING TREATMENT

Except in the patients mentioned as developing intercurrent infections, there was no appreciable variation in temperature, pulse, respiration or blood pressure. No patient developed a rash.

Of the patients receiving meprobamate, two gained up to 2 pounds in weight and four lost up to 9 pounds in weight. Of those receiving the control tablets, four gained up to 8 pounds in weight and two lost up to 7 pounds in weight. No abnormality was observed in the urine. No patient developed any blood dyscrasia.

FITS

No evidence was obtained to suggest that meprobamate affected the frequency of epileptic fits.

EEG VARIATIONS

As stated above, the EEG was repeated on those patients receiving meprobamate. In each of two cases the EEG was reported more abnormal than before they received meprobamate. In one of these cases delta activity, which had been widespread, became localized in the right temporal region and in the other case delta activity which had not previously been noted was recorded from the right temporal region. In this case there was also an increase in the amplitude of delta activity.

In one case the EEG was reported as showing a very slight improvement. Delta activity previously localized in the right temporal region having disappeared.

In another case, a slight reduction in generalized delta activity was reported.

In the four remaining cases, the EEG was not repeated either because the patient had not completed the trial or because she was too unco-operative.

ACKNOWLEDGMENTS

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REFERENCES

1. PENNINGTON, W. M., "The effects of six ataraxics in neuropsychiatric patients". Read at the American Psychosomatic Society Meeting, New York City, 6-8 October, 1955.
2. BORRUS, J. C., "Study of effect of Miltown on psychiatric states", *J.A.M.A.*, 1955, **157**, 1596.
3. LEMERE, M., "New tranquillizing drugs", *Northwest Med.*, 1955, **54**, 1098.
4. SELLING, L. S., "Clinical study of a new tranquillizing drug; use of Miltown", *J.A.M.A.*, 1955, **167**, 1594.
5. PERLSTEIN, M. A., "Miltown, its use in convulsive and related disorders", 1956, *J.A.M.A.*, April.

THE LEUCOCYTE COUNT IN CHILDREN WITH MONGOLISM

By

URSULA MITTWOCH, Ph.D.

Galton Laboratory, University College, London

A PREVIOUS communication (Mittwoch, 1957) contained evidence that the leucocyte count in children with mongolism showed certain abnormalities. Although the total count was not affected, the lymphocyte count was lower and the neutrophil count seemed somewhat higher than in a comparable control group. It will be shown in the present paper that these differences between mongol and non-mongol children are dependent on age, and that the gradual drop with age of the lymphocyte count, which is found in normal children (Kato, 1935), does not occur in children with mongolism.

PATIENTS INVESTIGATED

The investigation was performed on 50 children with mongolism and on 50 non-mongol, mentally defective children, who were all patients of the Fountain Hospital. The range of ages was between 3 and 14 years, except for one patient of 20 years in both groups. The mean age of all children with mongolism was 7.16 years, that of the controls 6.91 years. In both groups males and females were present in equal numbers, and, in order to keep conditions as comparable as possible, each time specimens of blood were taken from mongols, specimens were also taken from the same number of controls of the same sex and similar age.

METHODS

Specimens of capillary blood were obtained from pricks on the finger. All specimens were taken during the middle morning hours. For the counting of total leucocytes, 0.025 c.c. of blood were taken and diluted 20 times. Four counts of 1 sq. mm. on the haemocytometer were performed.

For the differential count, blood films were made on cover-slips, stained with Giemsa and mounted. Two hundred cells were classified for each count.

RESULTS

The mean numbers of the different types of leucocytes found in mongols and controls are shown in Table I. The last column of the Table gives the probability with which the differences between the mongols and controls would occur on a random hypothesis.

It will be seen that the figures for total granulocytes are very similar to those for neutrophils, so that it makes no difference which count is chosen. In the last row of the Table the difference between granulocytes and lymphocytes is given for each group. Although it is not implied that this difference between the granulocyte and lymphocyte counts has any physiological basis,

TABLE I

Mean Leucocyte Counts Per c.mm. of Blood in 50 Mongols and 50 Controls

Type of Cell	Mongols	Controls	Mongols- Controls	t ₉₈	Probability
Total leucocytes	8,254	7,965	289	0.53	0.6
Total granulocytes	5,492	4,402	1,089	2.69	0.01*
Neutrophils	5,184	4,048	1,136	2.84	0.01*
Eosinophils	282	339	-57	1.00	0.4
Basophils	26	15	11	$\chi^2=5.80^\dagger$	0.02*
Lymphocytes	2,102	2,888	-786	3.36	0.01*
Monocytes	660	674	-16	0.25	0.8
Granulocytes-lymphocytes ..	3,390	1,514	1,876	4.44	0.001*

* Difference judged to be significant.

† Owing to the small number of basophils, the distribution is discontinuous, and the χ^2 method had therefore to be used.

it was used because in practice it provided the biggest difference between the mongols and controls.

The total number of leucocytes found was very similar in the mongols and controls, and the small excess in the mongols is not statistically significant. Again, no significant differences between the two groups were observed in the numbers of eosinophils and in the monocytes.

The mongols differed in the number of neutrophils and of lymphocytes, as well as in the number of basophils; but the basophil count was so low (0.3 per cent. and 0.19 per cent. in mongols and controls respectively), that the numbers are difficult to interpret.

In the mongols there were more neutrophils and fewer lymphocytes than in the controls. These differences were particularly marked in the younger age groups as will be seen from Tables II and III. For the purpose of these tables both groups were divided into two according to age; the mean ages for the younger and older mongols were 4.47 and 9.85 years respectively, and those for the younger and older control groups 4.33 and 9.65 years respectively.

TABLE II

Comparison Between Leucocyte Counts in 25 Mongols (Average Age 4.47 Years) and 25 Controls (Average Age 4.33 Years)

Type of Cell	Mongols	Controls	Mongols- Controls	t ₄₈	P
Total leucocytes	8,768	8,416	352	0.44	0.7
Neutrophils ..	5,789	4,235	1,554	2.66	0.02
Lymphocytes ..	2,040	3,271	-1,231	3.45	0.01

TABLE III

Comparison Between Leucocyte Counts in 25 Mongols (Average Age 9.85 Years) and 25 Controls (Average Age 9.65 Years)

Type of Cell	Mongols	Controls	Mongols- Controls	t ₄₈	P
Total leucocytes	7,740	7,514	226	0.32	0.8
Neutrophils ..	4,578	3,860	718	1.35	0.2
Lymphocytes ..	2,164	2,505	-341	1.18	0.3

It will be seen, from Table II, that in the younger mongols there was an excess in the number of neutrophils of about 1,500, while the lymphocyte count showed a relatively even more marked shortage of about 1,200 cells. In the older age group (Table III) there was still an excess of neutrophils and a shortage of lymphocytes in the mongols, but the differences are no longer significant.

In both age groups the differences of the total white cell counts are quite insignificant. A comparison of the leucocyte counts between the younger and older group of mongols is given in Table IV, while a similar comparison for the controls is given in Table V. The most important point emerging from these tables is that while, in the controls, the lymphocyte count fell with age, this effect was not observed in the mongols.

TABLE IV

Comparison Between Leucocyte Counts in 25 Mongols of Average Age 4.47 Years and 25 Mongols of Average Age 9.85 Years

Type of Cell	Younger Mongols	Older Mongols	Younger Mongols—Older Mongols	48	P
Total leucocytes	8,768	7,740	1,028	1.03	0.3
Neutrophils ..	5,789	4,578	1,211	1.92	0.1
Lymphocytes ..	2,040	2,164	-124	0.37	0.8

TABLE V

Comparison Between Leucocyte Counts of 25 Controls of Average Age 4.33 Years and 25 Controls of Average Age 9.65 Years

Type of Cell	Younger Controls	Older Controls	Younger Controls—Older Controls	48	P
Total leucocytes	8,416	7,514	902	1.32	0.2
Neutrophils	4,235	3,860	375	0.80	0.5
Lymphocytes ..	3,271	2,505	766	2.25	0.05

DISCUSSION

It is sometimes asserted that, owing to the increased susceptibility of mongols to infections of various kinds, leucocytosis may be a prevalent condition. The present findings do not support this view. A few rather high counts were observed, but their number was not large. Thus three of the mongols had total white cell counts of over 14,000 per c.mm., as compared with two cases in the controls. None of the counts was over 15,500, and the average number of, roughly, 8,000 leucocytes per c.mm. in both the mongols and the controls is well within the range of that reported by other investigators for similar age groups (cf. Sturgis and Bethell, 1943). These findings therefore agree with those of Benda (1947) that leucocytosis is not a common factor associated with mongolism.

The most striking difference between the mongols and controls in the present series is the low lymphocyte count in the mongols belonging to the younger age group, i.e. between 3 and 6 years. This low lymphocyte count was associated with a rather high neutrophil count. Normally infants have a high lymphocyte count, which gradually falls with age. At the age of 4 years lymphocytes and neutrophils should be present in about equal numbers of, roughly, 4,000 per c.mm. (Whitby and Britton, 1953; Kato, 1935; Sturgis and Bethell, 1943). Thereafter the lymphocyte count continues to fall throughout childhood. As can be seen from Table II, the average numbers of neutrophils and lymphocytes in the younger group of control children (mean age 4.33 years) are 4,235 and 3,271 respectively, while the corresponding figures for mongols (mean age 4.47 years) are 5,789 and 2,040 respectively. The ratio of neutrophils to lymphocytes in the control group is therefore 1.3, while

that in the mongols is 2·8. It is thus evident that the neutrophil and lymphocyte counts in mentally defective, non-mongol children between 3 and 6 years is much closer to the normal than to the counts in mongols.

In the older age group the differences between the mongols and controls have become smaller. This is largely due to the fact that the fall of the lymphocyte count with age, which is found in normal children and which was observed in the mentally defective controls, did not occur in the mongols whose lymphocyte count was low throughout the whole age group investigated. Although mongols may be more retarded with respect to certain aspects of their chemical physiology than other mentally defective children (Stern and Lewis, 1957), a comparison of the leucocyte counts in mongols and controls does not suggest that the changes observed merely reflect a displacement of the changes in the leucocyte count with age. It seems more likely that a qualitative difference is involved. Since mongolism predisposes to leukaemia (Stewart, 1957; Krivit and Good, 1956; Merrit and Harris, 1956), it is felt that the cause of the abnormal leucocyte counts in children with mongolism is worth investigating.

The common occurrence of lymphopenia in children with mongolism was already described by Benda (1947). The data of Shapiro (1949) show no evidence that the percentage lymphocyte count of adult mongols is lower than that of controls. Both findings fit in with the present data, which indicate that the relative lymphopenia of mongol children compared with controls decreases with increasing age during childhood; it might therefore have disappeared altogether by the time adulthood is reached.

SUMMARY

Total and differential leucocyte counts were obtained for 50 children with mongolism between the ages of 3 and 14 years and for 50 non-mongol mentally defective children in the same age group.

The total leucocyte count showed no significant difference between mongols and controls. The mean value for mongols was 8,254, that for controls 7,965.

The neutrophil count of the mongols was significantly higher than that of the controls. The mean count for mongols was 5,184, that for controls 4,048.

The lymphocyte count of the mongols was significantly lower than that of the controls. The mean count for mongols was 2,102, and that for controls 2,888. The lymphocyte count in the controls fell with increasing age, while that of the mongols remained stationary.

The basophil count was low in both mongols and controls, but it was higher in the mongols.

The eosinophil and monocyte counts showed no differences between mongols and controls.

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REFERENCES

- BENDA, C. E., *Mongolism and Cretinism*, 1947. London: Heinemann.
 KATO, K., "Leucocytes in infancy and childhood", *J. Pediat.*, 1935, 7, 7.
 KRIVIT, W., and GOOD, R. A., "The simultaneous occurrence of leukemia and mongolism", *Amer. J. Dis. Child.*, 1956, 91, 218.
 MERRIT, D. H., and HARRIS, V. S., "Mongolism and acute leukaemia", *Amer. J. Dis. Child.*, 1956, 92, 41.
 MITTWOCH, U., "Some observations on the leucocytes in mongolism", *J. Ment. Def. Res.*, 1957, 1.
 SHAPIRO, A., "The differential leucocyte count in mongols", *J. Ment. Sci.*, 1949, 95, 689.
 STERN, J., and LEWIS, W. H. P., "Serum proteins in mongolism", *J. Ment. Sci.*, 1957, 103, 222.
 STEWART, A., "A current survey of malignant diseases in children", *Proc. Roy. Soc. Med.*, 1957, 50, 251.
 STURGIS, C. C., and BETHELL, F. H., "Quantitative and qualitative variation in normal leucocytes", *Physiol. Rev.*, 1943, 23, 279.
 WHITBY, L. E. H., and BRITTON, C. J. C., *Disorders of the Blood*, 1953. London: Churchill.

THE EEG AS A DIAGNOSTIC AND PROGNOSTIC AID IN THE DIFFERENTIATION OF ORGANIC DISORDERS IN PATIENTS OVER 60*

By

E. C. TURTON, M.D., M.Sc., M.R.C.P., D.P.M.

*Consultant Psychiatrist
Barrow Hospital, near Bristol*

INTRODUCTION

THE problem of the increased number of elderly psychiatric patients universally encountered needs no stressing. The purpose of this paper is to assess the value of the electro-encephalogram (EEG) as an objective aid to diagnosis and prognosis in these persons. The great majority of the psychiatric patients seen over the age of 60 are either suffering from an affective disorder, almost invariably depressive in nature, or from an organic dementia whether senile or arteriosclerotic. The former is normally considered of good prognosis and the latter doubtful or bad according to the degree of the dementia and confusion.

However, there is bound to be some overlap between the affective disorders and the dementias but the amount is far from sure and it is these "mixed" cases which are both of interest, but also difficult diagnostically. Kay, Roth and Hopkins (1955) felt that the numbers of this group were small, although stressing the far worse prognosis, but this is a by no means universal opinion, for instance—Noyes (1948).

In an earlier part of this work, Maggs and Turton (1956) outlined briefly the previous literature on the subject and found no definite EEG difference between a group of "normal" subjects aged 60 or over and a second group of patients suffering from a depressive illness, but showing no clinical evidence of dementia. This was still the case even when the depressive group was divided into those of early onset, i.e., before 60 years, and those of late onset.

It was desirable to establish two somewhat similar points. First, is the prognosis worse where clinically no dementia is present and the EEG abnormal? Second, is the prognosis better where, even though clinically some dementia is present, the EEG is normal?

MATERIAL AND METHODS

In the beginning of 1952 a long-term survey of elderly patients and their EEGs was started and, as far as possible, all patients admitted to a psychiatric hospital, taking primarily short stay patients of good prognosis, were examined over a five-year period.

The commonest cause of failure to obtain an EEG was excessive agitation, this being particularly marked in the women and accounting for the lower percentage examined. Either the necessity to give E.C.T. quickly or this treatment having recently been given, were the next commonest causes for no EEG being performed.

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All the patients had satisfactory tracings obtained from an Ediswan 8-Channel electro-encephalograph. Overbreathing was not routinely practised, as many elderly persons find it difficult to increase their respiratory exchange sufficiently. In those cases where it was performed, the results were not noteworthy and are not included. Photic stimulation was routinely used and in a number of cases changes that would normally be considered of an epileptic nature occurred. The records were all reported according to a fixed pre-arranged plan and without clinical knowledge of the case. The patients were not under the influence of any drugs, other than mild night sedation, at the time of the examination.

The diagnosis in the great majority was of a depressive illness and in many cases it was, as expected, recurrent and characterized by pronounced agitation. Taking the group as a whole, dementia was only rarely present clinically, treatment was usually E.C.T. and the duration of stay in hospital predominantly brief.

RESULTS

During the period commencing 1 January, 1952, and ending 31 December, 1956, a total of 670 patients, 260 males and 410 females, aged 60 or over, were admitted to the hospital. Of this total 482 (75 per cent.), 207 males (80 per cent.) and 275 females (67 per cent.), had satisfactory EEGs performed and stayed long enough in hospital for adequate clinical assessment.

Table I shows the distribution of the cases examined according to sex,

TABLE I
Distribution of Patients According to Sex, Age and Record Type

Total number of patients		..	..	..	..	..	..	482
Sex	{	Male ..	..	..	..	..	..	207
		Female ..	..	..	..	..	..	275
Age in years	{	60-69 ..	..	..	..	..	..	376
		70-79 ..	..	..	..	..	..	100
		80+ ..	..	..	..	..	..	6
Record type	{	Normal ..	..	..	..	..	..	211
		Borderline normal ..	..	..	..	..	..	142
		Slow wave changes ..	..	..	..	..	..	109
		Others ..	..	..	..	..	..	20

age and record type. The average age of the sample was not really high with 376 (78 per cent.) between 60-69, 100 (21 per cent.) between 70-79, and only 8 (1 per cent.) 80 or over. The number of borderline normal records showing predominantly fast activity is large—142 (29 per cent.). There are 109 (23 per cent.) with diffuse generalized slow changes and 20 (4 per cent.) with either epileptic or focal abnormalities.

Table II shows how the EEGs, considered abnormal with slow wave changes, are related to the clinical opinion. In 41 cases no clinical evidence of dementia was present; in 18—slight; in 30—moderate, and in 20—severe. There is a reasonable correlation between the more severely disorganized EEGs and the degree of dementia but it is quite clear that where the changes are less marked their significance in the light of clinical observation is, at the best, doubtful.

The eventual outcome of the 39 cases with abnormal records of moderate

TABLE II

Diffuse Slow Wave Changes Correlated with the Clinical Estimate of Dementia

EEG Generalized Slow Wave Changes			Clinical Estimate of Dementia			
			Nil	Slight	Moderate	Severe
Degree:						
(1) Moderate	..	..	39	17	29	10
(2) Severe	..	..	2	1	1	10

degree and a clinical estimate of no dementia did not differ significantly from the general favourable outcome. Thirty-six were discharged within six months, although two of these have since been re-admitted three times and one six times. Two more left within the year and one died in hospital at seven months of a coronary thrombosis. This is in complete conformity with the outcome of the rest of the group.

Table III shows the results of the clinical assessment of the degree of

TABLE III

Degree of Clinical Dementia, Outcome and EEG Changes

Degree of Clinical Dementia		Total	Good Outcome	Bad Outcome	Deaths	EEG
Moderate	..	37	10	22	5	Normal
		36	20	11	5	Abnormal
Severe	..	3	1	1	1	Normal
		21	5	8	8	Abnormal

dementia compared with the outcome in terms of normal and abnormal EEGs. Once again, where the dementia was clinically considered severe, there is a fair degree of correlation between the EEG findings and the outcome. Where dementia of moderate degree was estimated clinically, the outcome for the patient was better if the EEG was abnormal than normal, the finding being significant at the 5 per cent. level. This result should probably be disregarded.

Various other factors were considered but are not being given in detail because they were not found to be significant. There was no definite sex difference in either the percentage of normal, borderline or abnormal records. Hypertension and age, within the limits investigated, did not appear to be a cause of abnormalities. Elevation of either or both systolic and diastolic pressures, even if severe, was perfectly compatible with normal records and those with marked persisting hypertension did not show a greater percentage of abnormal records.

There was a tendency for those with low pressures to have abnormal records, and this is probably related to the fact that some of these were in heart failure and the pressure was showing reduction from its usual level.

Photic stimulation evoked changes which would normally be considered epileptic in 30 cases (6.1 per cent.) where there was clinically no evidence of epilepsy and where the record taken at rest was quite normal. In 13 the changes were very marked and persistent. However, this finding is almost certainly not of any special significance as related to old age and it is better to regard these changes as a measure of the convulsive threshold, as suggested by Gastaut

(1953), rather than as a sign of a specific epileptic tendency. Inevitably, however, it reduces considerably the clinical value of the provocative procedure.

SERIAL EXAMINATIONS

In all, 145 patients (30 per cent.) had 2 or more records and these were to some extent selected, representing mainly either those who relapsed and had to return to hospital, or those who were never discharged. The distribution of the repeat recordings in time is shown in Table IV. Of these, only 22 showed a

TABLE IV
Repeat Examinations

At Under	1 Month	6 Months	1 Year	3 Years	5 Years	Total
	23	37	15	52	18	145

significant change from the original one, being improved in 9, worse in 11, and fluctuant in 2.

Within a month a record taken from a patient suffering from a cerebral thrombosis had improved from a focal slow wave discharge to borderline normal. Within six months, four records had changed significantly without any corresponding clinical change. The other records which changed over longer periods of time again showed but little correspondence with the clinical state. Two patients had a persisting and prominent left anterior temporal delta focus over a period of five years' careful observation without change. It is somewhat surprising that so many of these patients retained similar records over the years, often showing little tendency to worsen or improve.

DISCUSSION

Taken as a whole, the results are disappointing. It is clear that in the majority of cases with severe dementia the EEG will be correspondingly abnormal, as was found by Weiner and Schuster (1956), and that these cases are the ones with the worst prognosis. But it is just these cases which are not a major diagnostic problem. Where there was only a moderate degree of dementia present clinically, this was a considerably better prognostic guide than the EEG changes. In contradistinction, a moderate degree of diffuse slow wave change in the EEG does not adversely affect the prognosis. Furthermore, a moderate degree of clinical dementia coupled with an abnormal EEG was found, somewhat improbably, to have a better outcome than in those cases where the EEG was normal.

Despite the fact that almost a third of the total had one or more repeat examinations, no further clinical information was obtained from these. Roseman *et al.* (1952) and Sheridan *et al.* (1955) had both stressed the increased value of serial recordings.

If the EEG changes are related to alterations in cerebral circulation, as suggested by Obrist and Bissell (1955), most of the inconsistencies in the results could be explained, and the fact that several patients with a blood pressure that had been reduced from a previously higher level with some associated heart failure, had abnormal records lends support to this theory.

CONCLUSIONS

1. Out of a total of 670 patients aged 60 years or over admitted during a five-year period, 482 had satisfactory EEGs performed and were clinically suitable for inclusion in the group.

2. There is only a poor correlation between EEG slow wave changes that are not gross, and a clinical diagnosis of dementia that is not severe. Hence the EEG is not a satisfactory weapon for routine use in attempting to assess the degree of early dementia in "mixed" cases, where affective components are also present, and ordinary psychiatric judgment still appears the soundest diagnostic and prognostic approach.

3. Where the EEG changes are severe, there is a reasonable correlation with the clinical state, but this is of little diagnostic or prognostic value, since these cases do not normally pose a clinical problem.

4. One hundred and forty-five patients had two or more records at varying intervals of time but there was no further significant information added.

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REFERENCES

- GASTAUT, H., *EEG Clin. Neurophysiol.*, 1953, Suppl. No. 4, 121.
KAY, D. W. K., ROTH, M., and HOPKINS, B., *J. Ment. Sci.*, 1955, **101**, 302
MAGGS, R., and TURTON, E. C., *J. Ment. Sci.*, 1956, **102**, 429.
NOYES, A. P., *Modern Clinical Psychiatry*, 1948. Ch. XV, 218. Philadelphia and London: W. B. Saunders & Co.
OBRIST, W. D., and BISSELL, L. F., *J. Geront.*, 1955, **10**, 315.
ROSEMAN, E., SCHMIDT, R. P., and FOLTZ, E. L., *Neurology*, 1952, **2**, 311-331.
SHERIDAN, F. P., YEAGER, C. L., OLIVER, W. A., and SIMON, A., *J. Geront.*, 1955, **10**, 53.
WEINER, H., and SCHUSTER, D. B., *EEG Clin. Neurophysiol.*, 1956, **8**, 479-488.

THE SPIRAL AFTER-EFFECT AS A TEST OF BRAIN DAMAGE*

By

H. C. HOLLAND

and

H. R. BEECH

Institute of Psychiatry, Maudsley Hospital, London

A NUMBER of studies have been recently concerned with the clinical application of the Archimedes spiral, an illusory negative after-effect of apparent movement, which requires no description here. Freeman and Josey (3) reported a relationship between the presence or absence of this after-effect and a clinical judgment of memory function, while a cross-validation study by Standlee (11) failed to confirm this observation when an objective measure of memory impairment was used.

More recently three contributions have appeared in the literature which suggest that inability to perceive the negative after-effect of the spiral is related to brain damage. The first of these studies (9) produced results indicating that the spiral might be an extremely useful instrument in the diagnosis of brain damage, 60 per cent. of organic patients failing to perceive the after-effect at all, while approximately 95 per cent. of functional patients and normal controls obtained the after-effect on all occasions.

Gallese (4), using a slightly modified procedure to that employed by Price and Deabler (9), attempted to validate the latter's findings. Five groups were tested: Normals; schizophrenics without organic involvement; organics having acute or chronic brain syndrome associated with alcoholic intoxication or idiopathic convulsive disorder; organics comprising C.N.S. syphilitics, encephalitics, circulatory disturbances, etc., and a group of lobotomized schizophrenics.

Gallese found that where a group of brain-damaged subjects similar to those used by Price and Deabler were employed, the number of identifications made were very similar in the two studies. However, 5 per cent. of his functionals failed to obtain the negative after-effect on any occasion. Even more striking was his finding that lobotomized schizophrenics, and those subjects whose brain damage was associated with alcoholism or convulsive disorder, tended to behave somewhat like normal controls.

A further validation study was conducted by Page *et al.* (7), who in addition to obtaining all-or-none reports from their subjects also measured the duration of the after-effect. As they give no quantitative data on the latter measure it is not possible to evaluate their reported finding that the measure did not discriminate between their brain damage and non-brain-damage groups. Their results on the all-or-none measure (i.e. occurrence or non-occurrence of the negative after-effect) were less striking than those cited by earlier investigators. Their data "... would suggest that 40 per cent. of the organic group would not be so identified. Conversely, some 15 per cent. of the non-organic patients would be inaccurately described as suffering cranial damage."

The study to be reported here represents an attempt to establish further the connection between organicity and failure to perceive the spiral after-effect.

As will be pointed out in the discussion, it is also possible to make predictions concerning the duration of the after-effect in brain-damaged subjects, and to this end this measure was added to the all-or-none measure which formed the basis of previous studies.

THE EXPERIMENT

(a) *The Groups*: Twenty-one brain-damaged, and 17 normal control subjects were used. The former comprised mainly patients whose history and symptomatology were associated with G.P.I., temporal lobectomy, convulsive disorder, cerebral atrophy, leucotomy, and traumatic brain injury. Seven of this group were female patients, while the control group was entirely male in composition. This male/female discrepancy between the two groups is probably unimportant for it has been shown that sex differences do not influence the results (7).

It has also been shown that age is not a significant variable (7) in the perception of the spiral after-effect, so that the difference between the mean ages of the organic and normal groups is unimportant. This conclusion is confirmed by the rank correlations between age and duration of after-effect for the two groups which were -0.29 for organics, and 0.05 for normal controls.

A further difference between the groups is probably in intellectual capacity. No data are available for the normal controls, but it is most likely that they were significantly brighter than the organic group for the former were all university students. Supporting the notion that intelligence probably plays little or no part in determining the spiral after-effect is the rank order correlation of 0.18 between the intelligence test score and after-effect duration for the brain-damaged group.

The relevant statistical data on the variables age and intelligence are given in Table I.

TABLE I
Brain-Damaged Group

Age		Sum Weighted Score	
Mean	S.D.	Mean	S.D.
33.14	12.60	26.29	9.73
Normal Controls			
29.47	6.87	—	—

It should be noted that intelligence was measured, in the organic group, by the sum of the weighted scores on three Wechsler subtests, Vocabulary, Similarities, and Block Design. This combination of subtest scores has been found to be a good estimate of scores obtained on the total of 10 Wechsler subtests (5).

(b) *Procedure*: The technical details of the administration and apparatus were very similar to those used by Price and Deabler (9) and it is not proposed to detail them here but merely to record the differences between their procedure and ours. These were as follows:

1. If and when the spiral after-effect was obtained the subject was simply asked to tell the experimenter when the after-effect ceased. The duration of this after-effect was measured in seconds.

2. The organic group received four trials, two with a clockwise motion of the spiral and two with an anti-clockwise motion. The controls, on the other hand, received only two trials, the first being a stimulation period of 15 seconds, and the second one of 30 seconds duration. Having determined that no significant differences existed between the duration of after-effect for the four 30-second stimulation periods for the organics ($F=1.253$), it was decided to compare the two groups on trial two where both had received a stimulation period of 30 seconds.

3. Certain minor changes in the physical conditions of stimulation were also introduced in testing control subjects (e.g. use of chinrest), but these changes have been shown not to influence the duration of the after-effect.

Results: The results do not fully confirm previous findings, for only one patient out of the 21 organics failed to perceive the after-effect on all four trials. One other organic failed to perceive the after-effect on three out of the four trials, but otherwise perfect scores were returned by other members of this group. No control subject failed to report the after-effect on each of the two trials given to this group.

However, a difference was found between the two groups on the duration of the perceived after-effect on trial 2. The relevant statistical data are shown in Table II.

TABLE II
Statistical Data. Duration of Spiral After-effect
Brain Damaged

Mean	S.D.
11.29	6.77

Controls

Mean	S.D.
19.70	6.93

$$t=3.763 \text{ (0.001 level)}$$

So far as identifications are concerned the results were not particularly striking. Eight out of 21 organics had an after-effect duration of less than 9 seconds (i.e. lower than any normal control), while 3 control subjects had after-effect duration scores of more than 27 seconds (i.e. higher than any organic.)

In spite of the poor differentiation on the all-or-none score, and the considerable overlap between the two groups on duration of after-effect on trial 2, these results do indicate that the spiral test may be useful in the diagnosis of organicity.

It might be objected that the measure which discriminated between our groups was based upon only one trial, but it is thought that this objection is not serious. It may be that further trials would have improved the differentiation, or worsened it, but it has been shown by Holland (6) and other authors that the spiral effect is an extremely reliable one.

DISCUSSION

In their recent paper Price and Deabler (9) put forward two possible explanations of the observed differences in the perception of the spiral after-effect. One of these explanations was based upon Shapiro's theory (10) which

had been developed as a result of investigating certain perceptual anomalies found among brain-damaged subjects. In order to account for his observations Shapiro postulated an intensification of cortical inhibitory processes in organic patients, and he was probably the first to make use of such a model in this connection.

A similar explanation of individual differences in the persistence of the spiral after-effect has recently been advanced by Eysenck (1). It will be remembered that he postulates as one of the main parameters of personality the dimension of Extraversion-Introversion. In its behavioural aspects this dimension is characterized by patterns of responses which are dependent upon the rate at which inhibition is aroused by the passage of neural impulses through the cortical pathways. Thus, for Eysenck, the behavioural dimension is paralleled by an inhibitory dimension whereby the introvert is characterized by low inhibitory potential and high excitation, and the extravert by high inhibition and low excitation. On this assumption he has been able to make a number of predictions concerning behavioural differences and, using a Hullian model, integrate certain facts in the fields of learning theory and personality.

Most of Eysenck's work so far has been conducted with normals and neurotics, but in the sphere of brain damage he has suggested (1) that organics, because of the intensification of their inhibitory processes, e.g. negative induction, should display patterns of behaviour and responses similar to those of the extreme extravert. A study conducted by Petrie (8), employing leucotomized psychiatric patients, gave results which support the notion that brain damage gives rise to more extraverted patterns of behaviour.

As a mainly central phenomenon, having a minor though measurable peripheral effect, the spiral after-sensation is readily testable in terms of Eysenck's theory. Two experiments have already been conducted and a third is at present under way. Of the two for which results are available the first is a drug study (2) in which the spiral test was administered to six subjects on each of three days. The experimental conditions were identical with the exception that the subjects had taken either d-amphetamine sulphate (10 mg.), sodium amylo-barbitone ($4\frac{1}{2}$ grains), or a placebo. The persistence times of the after-effect under these conditions differed significantly in the predicted direction, the same subject giving significantly shorter persistence times after amytal than under either placebo or the amphetamine. This strongly suggests that drugs which are cortical inhibitors enhance satiation effects and therefore affect the duration of after-effects which are a function of that process.

The second study is one in which an attempt was made to determine the relationship between a questionnaire measure of extraversion (the Rathymia scale of the Maudsley Personality Inventory), and the persistence of the spiral after-effect. The sample in this case was a group of normal university students (N 17) who were given six trials on the spiral at the following periods of stimulation: 100 secs. (one hour break), 15 secs., 30, 50, 80, 100 secs., with one and a half minute intervals between trials. In the same order, the correlation of the responses to these periods with the Rathymia score of the subjects were, 0.36, 0.28, 0.28, 0.34, 0.25, and 0.12. Although none of these correlations manage to achieve statistical significance they are, without exception, in the predicted direction.

From the evidence outlined above it would seem that there are reasonable grounds for assuming that the cortical inhibition theory offers some explanation of the individual differences which are characteristic of performance on the spiral test. However, an alternative explanation of individual differences in

duration of spiral after-effect is deducible from the same theory. Too often in psychology we assume that because we have gained the "co-operation" of the subject the effectiveness of the stimuli we administer is equal in all cases. One of us has reported (6) that the persistence of the after-effect appears to be directly related to the ability to maintain fixation, and that if the fixation point is changed randomly over the surface of the disc, during a one-minute period of stimulation, no after-effect is observed. It is often stressed in the literature on brain damage that the patients are very easily distracted from any task or test. A difficulty in maintaining fixation may therefore be contributing to the shorter duration of spiral after-effect in organic patients. This fixation difficulty may itself be a function of the growth of an inhibitory process, satiation of the stimulated cortical elements necessitating a change in fixation which may serve a useful biological purpose.

(It is interesting to note here that a significant correlation has also been determined between a measure of Extraversion and the ability to fixate.)

An interesting speculation based upon empirical observation may also be relevant in discussing the clinical application of the spiral after-effect. It may be that perception of the after-effect, whether on an all-or-none basis, or its duration, depends upon the type of damage sustained. Having regard to the results of this and other studies, together with the types of brain-damaged subjects used, leads the authors to suggest that while subjects with strictly localized lesions might behave like normal controls, patients with injuries subsumed under the category heading of "multiple and diffuse" might fail to perceive the after-effect. A study comparing various classes of brain damage would be necessary in order to check on this tentative suggestion.

SUMMARY

An attempt was made to validate certain previous observations which pointed to a relationship between brain-damage and failure to perceive the negative after-effect of the Archimedes spiral. The results of comparing scores for the duration of after-effect indicated that the length of this period was reduced in brain-damaged subjects, while scores on an all-or-none basis failed to confirm previous findings.

Two possible explanations for the results of this and other studies were put forward. First, individual differences in the degree of cortical inhibition, and differences in the type of brain damage sustained.

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REFERENCES

1. EYSENCK, H. J., "Cortical inhibition, figural after-effect, and theory of personality", *J. abnorm. soc. Psychol.*, 1955, **51**, 94-106.
2. *Idem*, HOLLAND, H. C., and TROUTON, D., "Drugs and Personality. 3. The effects of stimulant and depressant drugs on visual after-effects", *J. Ment. Sci.*, 1957, **103**, 650-655.
3. FREEMAN, E., and JOSEY, W. E., "Quantitative visual index to memory impairment", *Arch. Neurol. Psychiat.*, 1949, **62**, 794-797.
4. GALLESE, A. J., "Spiral after-effect as a test of brain damage", *J. clin. Psychol.*, 1956, **12**, 254-258.
5. HILDEN, A. N., TAYLOR, J. W., and DUBOIS, P. H., "Empirical evaluation of short Wechsler-Bellevue Scales", *J. clin. Psychol.*, 1952, **8**, 321-331.
6. HOLLAND, H. C., "The Archimedes Spiral", *Nature*, 1957, **179**, 432-433.
7. PAGE, H. A., RAKITA, G., KAPLAN, H. K., and SMITH, N. B., "Another application of the spiral after-effect in the determination of brain damage", *J. consult. Psychol.*, 1957, **21**, 89-91.

8. PETRIE, A., "A comparison of the psychological effects of different types of operations on the parietal lobes", *J. Ment. Sci.*, 1952, **98**, 326-329.
9. PRICE, A. C., and DEABLER, H. L., "Diagnosis of organicity by means of spiral after-effects", *J. consult. Psychol.*, 1955, **19**, 299-302.
10. SHAPIRO, M. B., "Experimental studies of a perceptual anomaly. III. The testing of an explanatory theory", *J. Ment. Sci.*, 1953, **99**, 394-409.
11. STANDLEE, L. S., "The Archimedes negative after-effect as an indication of memory impairment", *J. consult. Psychol.*, 1953, **17**, 317.

DEPERSONALIZATION AND ALLIED DISTURBANCES IN CHILDHOOD

By

D. J. SALFIELD, M.D., B.Sc., D.P.M.

*Consultant Psychiatrist
Derbyshire Children's Hospital and Child Guidance Service*

INTRODUCTION

DEPERSONALIZATION, derealization, unreality feelings, metamorphopsia and similar symptoms are not unusual in neuro-psychiatric patients. They are said to be extremely rare in children, Tramer (22) does not mention them, and Kanner (9) remarks only that "feelings of unreality are exceedingly rare in children. Things seem unreal to the patient, different from what he knows them to be. He is fully aware of the incongruity of his experience, which sometimes causes considerable distress." He quotes the case of a slightly retarded boy of almost 14 years who combined feelings of unreality and derealization with occasional macropsia.

Binswanger (5) reports retrospections of a physician, who at the age of between 11 and 13 had experiences of his bed, together with the bedroom, lengthening and broadening, into infinity. The ticking of the clock, his heartbeats sounded like very loud hammerbeats, and a fly had the size of a sparrow. I know of no more detailed accounts.

Adequate surveys with reference to adults are found in Critchley (6), Schilder (19) and Hécœn and Ajuriaguerra (8).

Case 1. A boy of 14, in good health, of average intelligence and, until recently, unremarkable personality, was referred because he had "peculiar feelings", for some months. At the time of examination they were present almost all the time. He said he felt not himself, had a sensation of pressure or mild ache all over the head and felt listless. The most accurate description I could obtain from him was that, for instance, returning home from school he felt he had not been there at all, that he had never left home, but knew perfectly well that he had in fact been to school. Often the world did not look as it usually does. He denied other symptoms, but was depressed, retarded, and emotionally dull. He day-dreamed a good deal and there was the suggestion of some incongruity of affect. I diagnosed a mild endogenous depression with depersonalization in a schizoid personality. His disease was of the adult type. He was pubertal and socially quite mature. Sedative and euphorizing treatment had little effect on the presenting symptom of depersonalization but relieved the depression. He attended no longer after about six months, during which he had made good progress; he was well for 1½ years and working, but has relapsed recently. His case is interesting only because of the age at which the disease started.

Case 2. I reported shortly the case of a girl of 15, of hysterical make-up, whose symptoms were of the depersonalization-derealization group. There was persistent fear of impending death, feeling of utter abandonment and helplessness, which could be traced to an early infantile experience of being in danger of suffocation. She had cried for help and no one came. After the analytical investigation her symptoms quickly disappeared and several months after discharge she was still well. Physical treatments (E.C.T., modified insulin and prolonged narcosis) had had no effect (12).

Again, this patient, essentially grown-up in soma and psyche, was unusually young for this type of disorder and, therefore, of interest.

Case 3. A girl of 10 was presented in October, 1955. She complained of headaches and double vision. The headaches had started 18 months ago. The parents said that the first one was accompanied by a rise of temperature to 101° F. The headaches had become worse in the three months before admission, in spite of glasses prescribed *ad hoc* 12 months before that. During those three months she complained of seeing double in the right eye, on looking to the right side. She had vomited the last two days. Two years ago she was "knocked out" for about a second. She has had periodic abdominal pains for an unspecified period. Examination including skull X-ray and C.S.F. were normal. But mother hinted at secret fears about menstruation instilled into the girl by another child. She was then, in January, 1956, transferred to my care, after I had seen her before in hospital. The parents then alleged that the girl's personality had changed from cheerful and outgoing to miserable and retiring. To me she complained of frequent paling and giddiness (with rotation). There was no longer any diplopia, there were no pains and, on physical examination, no signs of somatic disease. But there was profuse sweating. She revealed then that she had several times lately seen an ape coming out of the wall and a wardrobe open by itself. A voice kept telling her to be a bad girl. Otherwise there were no personality abnormalities. Special investigations: I.Q. (Revised Stanford Binet Scale) 100. Z-test (modification of the Rorschach inkblot test) appeared to be within normal limits both then and later in May. Her man-drawing appeared of inferior intellectual quality, and poorly integrated, reminding one of drawings of children with organic disease of the C.N.S. The EEG was abnormal, of a configuration found in "psychopaths". *Progress:* In March all symptoms were receding, and in May she was free from symptoms and seems to have remained so. She was treated by mild sedation only.

Discussion. There were "true" hallucinations, both auditory and visual. But hallucinations in childhood need not have the same ominous quality as in adults (2 and 22). They may be part of an hysterical, compensatory, mechanism. Headaches, vomiting and sweating might be somatizations of anxiety via the autonomic nervous system. For the monocular diplopia there were no local ocular causes. Bender (quoted from 6) draws attention to monocular diplopia and polyopia as a rare consequence of visual inattention, which points to an occipital lesion.

I am inclined to assume a toxic or infective, probably unilateral, cerebral disturbance of a transient nature which led primarily to a disturbance of visual perception through visual inattention. This may secondarily have produced psychosomatic symptoms as suggested. The probable key to the development of these secondary symptoms was a loss of the normal "position" and "equilibrium" in the world. This generated compensatory hostile feelings (cp. her auditory hallucinations) and expressions of disgust (vomiting). But it is impossible to verify these assumptions.

Case 4. Pat, then 9, woke one night in October, 1954 screaming and told her parents she felt "funny". Mother panicked, father slapped her, and after two or three hours she calmed down, only to repeat the same scene in the morning. After two days' almost continual screaming she was admitted to hospital, became calmer after a week but again complained about "funny" feelings, tickling in her feet, which desquamated after one week. In February, 1955, a children's psychiatrist was consulted, as she was terrified of the dark and had fears of imminent death. She was conscious of her heartbeats and breathing and had peculiar sensations in her vulva. Mother, he diagnosed, was a chronic anxiety neurotic, and father took little interest in the child. She suffered from an anxiety state and was very dependent on her mother. She improved considerably but she still required mother to sleep in one bed with her, as this was the only way of allaying her nocturnal anxiety. A residual state of anxiety was then diagnosed, and removal of the child to a school for maladjusted children was suggested. This advice precipitated a panic reaction in the child. She feared her breathing would stop, she would suddenly die. She refused to go to school and was generally "difficult". Shortly afterwards she came under my care, i.e. in June, 1955.

I found her very anxious. She was afraid of going to bed or sleeping on her own, would not venture to school or clinic unaccompanied. But what terrorized her most were "peculiar" genital sensations. A severe, chronic anxiety state with depersonalization symptoms was diagnosed. This was an over-simplification. The whole personality was disordered and she had regressed to such an extent as to suggest, but for the good façade, that a psychotic process might have existed. She was excessively cautious with an almost paranoid tinge and presented a partial emotional infantilism and dependence of a quality commonly seen only in schizophrenics and similar psychotics.

Progress. Treatment was started by mild sedation; continued for over six months by psychotherapeutic means, viz. the method of "Paintings and Stories" (13). The psychological development ensuing is to be published separately. By September, 1956, the only symptom was fear of sleeping alone. In January, 1957, she decided it was "unhealthy" for a child to sleep with an older person and so gave up her last symptom. Over a year later, she is still well.

Discussion. The precipitating cause of the disorder was not detected. The exfoliation of her skin is peculiar, perhaps analogous to sudden loss of hair after emotional-vegetative crises. Undoubtedly her mother-dependence, and her father's relative indifference have much to do with the selection of the symptoms: refusal to go to bed alone and the "sensations" in the vulva. These may suggest a fear of, and a wish for, sexual assault. It is conceivable (as is supported by her further psychological development, as reflected in the "Paintings and Stories") that she had really experienced, a belated oedipal crisis. To forestall the criticism that she was not "depersonalized" but had "only" conversion-hysterical symptoms, I contend that there is not so much difference between these as may at first appear. Depersonalization is characterized by the feeling that one has fundamentally changed or feels strange. This often accompanies hysterical and other states of long duration in which there are perhaps via changes in the body schema dissociative personality changes. In this girl's case, the feeling of passivity was especially significant. Some inexplicable, strange feeling came over her, a feeling that was foreign. This, I think, constitutes the essence of depersonalization (18). As usual in depersonalization, there was a good deal of rumination.

Case 5. (This case is also published in German in "Psychotherapie", Bern, fully (17).) Sandra, eleven years, came to the medical out-patients in October, 1956 complaining of not seeing well at times. She had often, perhaps once daily, felt faint, without warning; she had done so since her fourth year. Then objects appeared hazy and smaller than normal in size. The faintness disappeared when she closed her eyes, and she felt better lying down. There was no vertigo, sweating or change of colour. But she had flashes in front of her eyes. Attacks lasted one to two hours. In the intervals she had vertical headaches. She never vomited or lost consciousness, and she remained coherent. She had no paraesthesiae, weakness, or twitching. She slept and ate well. Her bowels were regular. She could see and hear well. Her gait was normal; and apart from a soft systolic apical murmur there were no physical findings, special attention having been paid to the C.N.S.

She was an only child. She was delivered by forceps, but was uninjured. Milestones were reached normally. School work was average. There was no family history of any interest. X-ray of skull was normal. EEG was "slightly abnormal", showing a generalized mild dysrhythmia without diagnostic features. A provisional diagnosis of sensory epilepsy was made, and she was given $\frac{1}{2}$ gr. phenobarbitone t.d.s. Thereupon the number of attacks increased. She began to complain that things did not look real and that they looked "far-away". She could not see distant things clearly. She would not get up from her bed. She was afraid to go out, refused to go to school and felt she could not think clearly. On one occasion she complained of not being able to stand and had to be carried into the house. She was then admitted to hospital for further investigation. Physical examination again revealed no abnormalities, routine laboratory studies were normal. She still complained of being "far-away", but in hospital the number of attacks decreased to about one a day.

At this stage she came under my care (November, 1956). She told me that her eyes felt queer during the attacks. Things looked "different" then. She was positive that there was no blurring or indistinctness. People's movements were puppet-like. They "felt" different. She said she had had also "queer" feelings in her legs at times. She told me of a nocturnal attack, when she felt she could not breathe, but in fact, she said, she could do so perfectly well: she only felt she could not get enough air.

As to her personality, she was tense, anxious and extremely tremulous, but co-operative. She admitted that as an only child she was spoiled at home. She was of good average intelligence. The parents were what I think typical for an over-indulged child: indecisive, demanding, over-protective and, in their attitude to the doctor, passive-expectant. There was a striking difference in the girl when interviewed alone. She was then friendly, communicative, and eager to

help herself. When with mother, both were sullen, blank-looking, "united", uncommunicative. As the child improved, mother became reluctant to bring the child, finding appointments inconvenient.

Diagnosis. Anxiety-hysteria with mild derealization and depersonalization in an immature over-indulged and over-dependent child with a devouring mother.

Progress. Whilst still in hospital she was started on a type of autohypnotic therapy (14, 15, 16, 21). After 14 days, derealization receded. There was still some "hard feeling" in the eyes. "She appears much better generally, now plays and mixes well. Less reserved in ward", according to the house physician's observations. After another 2 weeks, she was symptomless and was discharged. She has been symptomless for six months now (July, 1957) excepting for an estimated 20 seconds in March, when in the street playing she "could not hear" and her "eyes felt funny", but she recovered before she found her mother.

Discussion. That this child's symptoms are parts of a well-developed chronic depersonalization syndrome on an anxiety-hysterical basis can hardly be in doubt. As to the symptoms of micropsia I refer to Critchley (loc. cit.) and Mueller (11). As to the rationale of the treatment I should add that Schilder (20) and Bender (3) have shown the importance which motor development has on the building up of the body schema and the whole personality. Whatever the precipitating causes for feelings of depersonalization, derealization, etc., it is the comparison of perceptions with pre-formed cerebral engram-patterns (e.g. body image) that brings about a feeling of familiarity or otherwise (cp. 1 and 10). Bender and Keeler (4) believe that E.C.T. dissolves the body image. The amorphous state makes new constructions possible. This re-organization may give rise to a more integrated body image. Cutner (7) analyses the patient's personality "expressions" in bodily features, tensions and feelings and is able to modify them. Our method might mobilize some components of the body image, and, as Bender says, make new constructions possible. The method is also valuable in cases in which other apperceptive disturbances may be thought to exist (cp. 17).

(Apperception I define as the experience of apprehending a "new" perception as understood and familiar, which occurs when it is "compared" with some "old" engram.)

CONCLUSION

Depersonalization and similar states are practically intractable in adults; although naturally remitting. They seem to be less difficult to treat in children. This may be due to a greater mobility of the child's body image (18).

SUMMARY

Several cases with symptoms such as depersonalization rarely seen in children are described; sometimes successful treatments are indicated.

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REFERENCES

1. BARUK, H., "Le sentiment de la personnalité, la dépersonnalisation et la cénesthésie", *Ann. Médico-psychologiques*, 1951, 2, 4.
2. BENDER, L., and LIPKOWITZ, H., *Amer. J. Orthopsych.*, 1940, 10. As reprinted with additions in *A Dynamic Psychopathology of Childhood*, chapter on Hallucinations in Children, 1954. Springfield.
3. *Idem*, "The Psychology of Children with Organic Disturbances of the Cerebellum", in Bender, L., *Psychopathology of Children with Organic Brain Disorders*, 1956. Springfield.

4. BENDER, L., and KEELER, W. R., "The Body Image of Schizophrenic Children following Electro-shock Therapy", *Amer. J. Orthopsychiatry*, 1952, **22**, 2.
5. BINSWANGER, L., *Z. Neurol.*, 1933, **145**, 598. Quoted from Karl Jaspers *Allgemeine Psychopathologie*, 1948. Berlin.
6. CRITCHLEY, M., *The Parietal Lobes*, 1953. London.
7. CUTNER, M., "On the Inclusion of certain 'Body Experiments' in Analysis", *Brit. J. Med. Psychol.*, 1953, **26**, 324.
8. HÉCÆEN, H., and AJURIAGUERRA, J. DE, *Méconnaissances et Hallucinations Corporelles*, 1952. Paris.
9. KANNER, L., *Child Psychiatry*, 1948. Oxford.
10. KRAPE, E., "Sur la dépersonnalisation", *Encéphale*, 1951, **11**, 3.
11. MUELLER, CHR., *Mikropsie und Makropsie*, 1956. Basel.
12. SALFIELD, D. J., "Notes on Psychotherapy of Children jointly with their Parents", *Z. Kinderpsychiat.*, 1951, **18**, 2.
13. *Idem*, and GREENLAND, C., "Painting and Stories, a diagnostic and therapeutic technique in child psychiatry", *Z. Kinderpsychiat.*, 1953, **20**, 4.
14. *Idem*, *Concentrative Relaxation*. To be published shortly.
15. *Idem*, "On Two Important Indian Contributions to Modern Psychotherapy", *Psychotherapy*, 1957, No. 3-6, Vol. I and II.
16. *Idem*, "Auto-hypno-therapeutic Techniques", *Brit. J. Med. Hypnot.* To appear January, 1958.
17. *Idem*, "Autogenes Training eines Kindes mit Depersonalisation", *Psychotherapie (Bern)*. To appear shortly.
18. SCHILDER, P., *Image and Appearance of the Human Body*, 1951. New York.
19. *Idem*, "The Psychological Implications of Motor Development", in Bender, L. (20).
20. *Psychopathology of Children with Organic Brain Disorders*, 1956. Springfield.
21. SCHULTZ, J. H., *Das Autogene Training*, 1956. Stuttgart.
22. TRAMER, M., *Lehrbuch der Allgemeinen Kinderpsychiatrie*, 1949. Basel.

MENTAL HOSPITAL PLANNING IN DENMARK

By

P. G. AUNGLE, M.R.C.P.E., D.P.M.

Senior Medical Officer

Royal Edinburgh Hospital for Mental and Nervous Diseases

By 1975 Denmark should have 9 new mental hospitals, each, with one exception, closely linked to a general hospital. This is the most striking of the proposals contained in the Report* of a Commission set up by the Danish Ministry of Internal Affairs in 1952. This Commission was to "report on trends of development of the State Mental Hospital Service in respect of organization and buildings, with special reference to the necessary adaptation of the hospitals to present-day needs and forms of treatment. The Commission's deliberations, which should principally be concerned with the modernizing and extension of the hospitals together with their position in relation to the general hospital service, should also include the possible extension of treatment to categories of patients with mental disorders other than true insanity. The Commission's work should be so arranged that its report can constitute the immediate foundation for the practical organization of future reform work." This Report contains much of general interest and much which might merit wider application.

Before reviewing the Commission's Report, a few explanatory words about hospital administration in Denmark are necessary. With a few exceptions, the general hospitals are under municipal or county authority administration, whereas the mental hospitals (with one exception) are state-administered, the Directorate of Mental Hospitals coming under the jurisdiction of the Ministry of Internal Affairs. In all medical matters the Directorate must consult the Central Health Board, which is itself subordinate to the Ministry of Internal Affairs, and functions in a purely advisory capacity. There are seven State mental hospitals, and one municipal mental hospital; the latter, St. Hans Hospital, serves exclusively Copenhagen.

The Commission assumed that the essential problem was not that of simply providing more mental hospital beds so that the mental hospitals could then function along traditional lines without the handicap of a bed shortage. Rather was it that of adapting the mental hospital system to meet a new situation, which has been arising during the last few decades, created by the increasing emphasis on active treatment, and which requires, for example, a different utilization of the space in existing hospitals. This argument is not a new one, but it seems to have been followed more thoroughly to its logical conclusion in the Danish plans.

The Report opens with a good historical account of the development of psychiatric treatment—an account evidently aimed at the lay reader. Then follows a review of the structure of each of the State mental hospitals. Of these, only four were built as mental hospitals, the last in 1915. The remaining three were created by conversion of other buildings, including an old prison (dating from 1743). The mental hospital statistics for 1955 show overcrowding up to 20 per cent., with an average of 9 per cent. This may be compared with

* "Betaenkning vedrørende statens sindsygevaesen" (Betaenkning nr. 165), Copenhagen, 1956.

the English figure (calculated from data in the Board of Control's Report for 1955) of 15 per cent. overcrowding.

Since it is out of the question to scrap the existing hospitals in the foreseeable future, it is proposed in the cases of the four hospitals built as such, that their "normal" bed-capacity should be reduced by 30 per cent. as a step towards reorganizing them along more modern lines. Of the other three, one is no longer to be used as a mental hospital, the ex-prison is to be rebuilt, whereas no change is required in the third—a converted palace. These changes will involve reducing the present number of beds in the State mental hospitals—5,700—by about 1,800.

The figure of 5,700 is remarkably small for a population of 3·5 million (i.e. the population of Denmark outside Copenhagen), giving only 1·6 beds per 1,000 population. This figure, however, has to be viewed in the light of two additional factors. The first is the use in Denmark, since 1915, of special after-care institutions which take from the mental hospitals patients who still require institutional care but not daily psychiatric supervision. These institutions call on local practitioners for the ordinary medical care of their patients and are visited, usually once a week, by a psychiatrist from the mental hospital which the institution serves. There are 10 such institutions, containing about 1,300 patients. The second factor is that the boarding out of mental patients is still used relatively extensively. Since the Second World War, however, social factors have led to diminishing possibilities in this field, and whereas over a thousand patients were cared for in this way in 1945, there were only 770 in 1955. It is justifiable to combine the figures for boarding-out and after-care institutions, since the former provision is likely to decline further, the deficiency having to be made up by increasing the number of beds in after-care institutions. These two provisions together account for 26 per cent. of the total "beds" available in the State psychiatric service, but the Commission proposes to raise this figure to about 30 per cent., since the number of beds in the after-care institutions is inadequate, and the institutions themselves are so distributed that some of the existing mental hospitals are poorly served in this respect.

Turning to the question of what new building is required the Report sets out the premises on which its proposals are based. The figures show that in 1950 there were 2·17 beds (of all kinds) per 1,000 population (outside Copenhagen), and it is proposed to raise this figure to 2·5 per 1,000 (or 3·5 per 1,000 population over the age of 15, if it is calculated on the basis of the population at risk). The population concerned being 3·5 million, this reform will involve the provision of a further 1,160 beds. The figure of 2·5 per 1,000 is adopted as a minimum, and is low in comparison with the English Ministry of Health recommendation (1950) of 4 mental hospital beds per 1,000.

The proposals for new building avoid piecemeal planning aimed only at correcting present deficiencies. Instead, plans are put forward for those needs which can be foreseen up to 1975. Assuming that the population will thereafter continue to increase at its present rate, it should then be sufficient to provide 90 new beds annually to keep pace with current needs.

Of the future needs on which such plans would have to be based, those related to population factors are most easily predictable. The future growth of population is considered in relation to both urban and rural communities, in view of the fact that the incidence of admission to mental hospitals is higher in urban communities. Urban communities are here defined as those with a population of at least 2,500; this figure has been chosen on the basis of Malzberg's

finding (1935) that the curve of admissions to mental hospitals shows a sharp rise for communities of over 2,500. Extrapolation of the Danish census figures shows that in the period 1950–1970 the rural population at risk (aged over 15 years) can be expected to decrease by 4 per cent. (from 1·32 to 1·27 million), whereas the urban population (aged over 15 years, outside Copenhagen) will increase by 45 per cent. (from 1·24 to 1·80 million). These trends will accentuate that part of the increasing need for mental hospital beds which is directly due to progressive ageing of the population. This conclusion is based on the present-day experience that the possibilities for home care of the milder senile confusional states and dementias are less in an urban than in a rural community.

The problem of the increasing number of old people actually in mental hospitals is shown, however, to be due principally to the striking fall in the death-rate of chronic patients. In 1937 the proportion of patients in the State mental hospitals who had been there more than 10 years was 35 per cent., whereas in 1952 it was 51 per cent. Indeed, a Norwegian investigation (Ødegaard, 1952) has estimated that 65 per cent. of the increase in the need for mental hospital beds in Norway in the period 1930–48 was due to this one factor. Admissions of patients over 60 to Danish mental hospitals have constituted a fairly steady proportion (10–12 per cent.) of the total, although their actual number has risen steadily—this trend being offset by a corresponding rise in the turnover of mental hospital beds.

In addition to these factors, which can be assessed quantitatively, others of a more qualitative kind are pointed out. For example, improvement of the State Mental Hospital Service will tend to increase the demand for beds. When modern or modernized hospitals free of overcrowding are available, both patients and their relatives will presumably be more willing to accept the prospect of admission. Furthermore, figures are given showing that the rate of admission falls with increasing distance between the hospital and the patient's home. The proposal to build several small hospitals rather than a few large ones, so that each hospital serves a relatively small region, should therefore lead to increased demand for admission. On the other hand, the out-patient treatment of marginal cases, instead of their admission, is more feasible when the region served by the hospital is small, so that these two factors may in fact tend partially to cancel each other out.

Finally, the proposed linkage between mental and general hospitals should reduce lay, not to mention medical, prejudice against the mental hospitals.

As mentioned before, the plans aim at meeting demands up to 1975. This period is evidently chosen as a compromise between the greater uncertainty involved in taking a longer period, and the very high rate of capital expenditure which a shorter period would involve.

On the basis of those factors which are quantitatively predictable, it is proposed to provide an extra 4,800 beds (3,200 mental hospital and 1,600 after-care institution beds)—1,800 of these compensating for the deficiency created by modernizing the existing hospitals.

The Report discusses at length the less costly alternative of establishing psychiatric wards within the general hospitals. This alternative is rejected on the grounds that beds so provided would not relieve the mental hospitals sufficiently, and also the latter's isolation would remain unchanged. It should, however, be made clear that the Commission is not opposed to general hospital psychiatric units on principle, but only as a substitute for new mental hospitals. Psychiatric units in general hospitals are, in fact, a well-established feature in Copenhagen,

which has 62 such beds per 100,000 inhabitants. This figure may be compared with Blacker's provisional proposal (1946) of 10 per 100,000.

Union of the new mental hospitals with general hospitals is accepted as axiomatic. It is hoped, by this means, to establish a day-to-day collaboration between psychiatrists and their colleagues in other clinical fields, and to strengthen the trend of latter years towards abolishing the artificial isolation of psychiatry from the rest of clinical medicine. The economic advantage of the mental hospital being able to share various facilities with the general hospital is obvious, and it is hoped that the close link with general hospitals will also ease the problem of obtaining nursing staff.

In Denmark there is no training for mental hospital nurses which is distinct from general nursing training. Trained mental hospital nursing staff is recruited from those who are doing, or have completed, their general training. At present, a nurse in general training may elect to work in a mental hospital for 6 months but it is intended to replace this by a compulsory period of 4 months.

It is stressed that all these advantages can only be achieved if the mental hospital is built close to the general hospital. The new mental hospitals are thus to be built adjacent to general hospitals, but, as mentioned earlier, there is to be one exception. One of the new hospitals is being built on a somewhat isolated site in North Jutland and is intended for those with illnesses of long duration, especially in the older age-groups. The advisability of such a project is questionable; it was originally planned as an ordinary mental hospital before the Commission was set up, but even though the site was clearly unsuitable it proved to be impossible to drop the project, hence the present compromise.

It is clear that if the new hospitals are to be closely linked to general hospitals, they must be smaller than those built hitherto, but, on the other hand, they must be large enough to permit the necessary degree of subdivision into different types of ward, and the Commission proposes 300-350 beds as the best compromise. According to the specimen plans included in the Report, a new mental hospital is to be so arranged that staff accommodation and convalescent wards will be nearest to the adjoining general hospital, followed by the admission wards, administration offices and special investigation departments, and with the wards for long-stay patients furthest away. Whereas pavilions are proposed for chronic patients, present-day requirements for admission wards, treatment, investigation and administration departments are more adequately met by a more concentrated kind of building. Nevertheless, in the latter case, single-storey buildings, with their more pleasant and non-institutional appearance, have been used, grouped in such a way that they can be interconnected by short corridors. Well away from the main part of the hospital is a child psychiatric clinic, with a small in-patient unit for acutely disturbed children.

The largest ward contains 24 beds, but this number is reduced to 16-18 in the admission wards, and 4 beds per room is the maximum. In addition to 10 admission wards, there are 3 convalescent wards and 8 wards for chronic and more severely disturbed patients. These plans are reproduced with English text in an article (1956) by Professor le Maire, Director of State Mental Hospitals in Denmark.

Eight new after-care institutions are to be built, with from 80 to 250 beds, and placed as near as possible to the "centres of gravity" of the populations they will serve.

Besides the planning of new building, the Report also discusses problems presented to mental hospitals by certain groups of patients. Treatment of the criminal insane, for example, is a particularly vexed question in Denmark.

There is no separate mental hospital corresponding to the English Broadmoor and all insane criminals are treated in the ordinary mental hospitals, one of which, it is true, has a special security unit with 30 beds for the most dangerous. Even in the case of this special unit, patients are transferred to an ordinary ward if their state improves sufficiently, and it is used only for male patients. It may be mentioned that this unit originally had 50 beds and, until as recently as 1955, was usually full. In the last 2 years, however, the number of patients has fallen to 25-30; this change is apparently due to the extensive use of chlorpromazine and reserpine, which has made it possible for many of these patients to be transferred to ordinary mental hospital wards.

The system described above is flexible and humane, but also has disadvantages. In practice, these patients come into one or other of two categories: either they are sentenced to psychiatric treatment during which admission to and discharge from hospital are at the psychiatrist's discretion, or they are sentenced to "detention" in a mental hospital. In the latter case, not only is the question of discharge decided by legal authority, but the hospital is expected to perform two distinct functions—treatment of the offender in accordance with medical requirements, and his detention in accordance with juridical requirements. Although these patients are relatively few—25 to 30 a year—many Danish psychiatrists feel strongly that the enforcement of a degree of detention, which would not otherwise be indicated on medical grounds, hampers the current trend towards greater freedom for mental hospital patients in general. It is maintained also that the presence of these patients reinforces lay prejudice against mental hospitals. To these arguments the Commission itself adds the point that the criminal patient himself suffers if the juridical demand for secure detention means, as it may do, that he must be kept in the environment of a closed ward when in fact his mental state justifies greater freedom. Nevertheless, considering that many of those sentenced to detention in a mental hospital have committed serious crimes, including murder, the attitude of the legal authorities seems on the whole conciliatory and sympathetic. In fact, the Danish Ministry of Justice prefers the present arrangement to setting up a special criminal mental hospital. The Commission on the other hand, proposes a special institution for those insane criminals who present particular security problems.

Other problems, including staffing problems, are touched on, and one or two of these may be worth mentioning. For example, only a very few of the mental hospital male nurses are at present fully trained and male nurses have played altogether a much more subsidiary role than in Britain. This tendency is to be reversed in order to help to meet the increasing need for nursing staff. However, the problem of nursing staff will presumably be a formidable one. According to private opinion, only 50 per cent. of the staff of the new hospitals is likely to consist of those who have completed, or are engaged in their full training.

The question of medical staffing is reviewed, but no proposals are given concerning the staffing structure to be adopted in the new hospitals, although, as the Commission itself states, the solution of the various staff problems will determine the standard of treatment and care in the new hospitals.

Appropriately, in view of the improved status which the mental hospitals are to have, the Commission suggested reform of the procedure for certification. According to Danish law, certification of a patient must be sanctioned by the local Chief Constable or, in his absence, by one of his deputies, and the transport of the patient to hospital is a police responsibility. It was particularly the latter feature which the Commission wished to have changed, but this

proposal was rejected by the Ministry of Justice. In fact, the element of deprivation of individual liberty involved in admission to a mental hospital does not seem to be taken quite so seriously in Denmark as in Britain. For example, a voluntary patient who intends to discharge himself can be detained at the discretion of the medical superintendent, subject to the Minister of Justice's consent and subject to appeal to a Court.

Finally, what are the prospects of these plans being fulfilled? The proposed building will involve an annual expenditure of £1,000,000 until 1970, thereafter £600,000–700,000 annually until 1975; it is unlikely that the Commission's plans will stand unaffected by economic factors for 20 years, but it is hoped that the building planned for the next ten years will be carried through, by which time the greater part of the modernization of the existing hospitals should be complete.

Some indication that these problems are being tackled in earnest is afforded by the fact that nine months after the Commission was set up, it put forward detailed proposals for dealing with what it considered to be the most acute problems. This interim report proposed the building of two new mental hospitals, and two new after-care institutions, to be followed by modernization of the most obsolete of the existing hospitals—involving in all an expenditure of £3,300,000. These proposals were accepted in their entirety and the necessary law was passed in March, 1953.

The two after-care institutions were opened in April, 1957, and it is clear that they have been thought out with great care. A possible criticism is that the same upper limit of 4 beds per room, which has been laid down for the new hospitals, has been applied to these after-care institutions, and when an institution of this kind is to take a considerable number of senile patients, the large number of small rooms will increase the problem of supervision.

The modernization of the most obsolete mental hospital—that based on an old prison—is in progress, and the prison building itself, a forbidding structure which dominated the rest of the hospital, is being demolished.

The first new hospital was to be finished by October, 1957. The second is being built together with a new general hospital. It has been possible in this case to plan the two hospitals as a combined whole from the beginning, so that when they are completed in 1960, they will provide the best attainable model of the new principle in the Danish State Mental Hospital Service.

SUMMARY

The Report of a Danish Commission which was set up in 1952 to examine the present deficiencies and plan the future reform of the Danish State Mental Hospitals is reviewed. The plans put forward aim at anticipating needs up to 1975 and include the building of 9 new mental hospitals, with 350 beds each, closely linked to general hospitals and also 8 new after-care institutions. A summary is given of the premises on which these plans are based.

Other proposals, including those for the criminal insane, are mentioned.

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REFERENCES

- BLACKER, C. P., *Neurosis and the Mental Health Services*, 1946. London, p. 63.
 BOARD OF CONTROL, *Annual Report to the Lord Chancellor for the Year 1955*, 1956. H.M. Stationery Office.
 LE MAIRE, L., *Mental Hospitals*, 1956, 7, 22.
 MALZBERG, B., *Psychiat. Q.*, 1935, 9, 55.
 MINISTRY OF HEALTH, *Development of the Consultant Services*, 1950. H.M. Stationery Office.
 ØDEGAARD, Ø., *Acta psychiat. et neurol. skandinav.*, 1952, 27, 353.

THE MUNROE CHECK LIST: A NOTE ON ITS VALIDITY IN CLINICAL RESEARCH

By

L. R. C. HAWARD, B.Sc., M.A., D.Psy.(Leyden)

*Senior Clinical Psychologist
Winterton Hospital, Sedgfield, Co. Durham*

and

K. MARSZALEK, Dip.Psych.(London)

*Clinical Psychologist
St. Luke's Hospital, Middlesbrough, Yorks*

INCREASING use of the Rorschach Inkblots Test is being made in medical diagnosis, not only in psychiatry, but in neurology as well. Bibliography is too extensive to quote here, and the reader is referred to the standard work by Klopfer *et al.* (1956) which lists nearly 3,000 references specific to Rorschach literature. The inclusion of a long chapter on the clinical use of this test in the latest research monograph of the Association for Research in Nervous and Mental Diseases (1954) is evidence of the increasing esteem in which this test is held by specialists in neurology and psychiatry, supported by the growing use of the test in diagnosing intracranial organic pathology. It is interesting to note that in the latter field the Rorschach has proved superior in diagnostic accuracy to any other single neurological test, such as EEG, lumbar puncture, ventriculography, etc., and is often as good as the complete battery together.

Such level of accuracy can only be attained by the skilled user, and diagnostic interpretations by individuals possessing an inadequate background in basic psychology and limited clinical experience are not only of questionable validity but can prove extremely dangerous.

Because of this, a trend has been noted in recent years towards the adoption of short-cut methods of analysis, usually of the "check list" or "diagnostic indices" type of scoring. Attempts have been made to justify these contractions of the standard scoring procedure on the grounds of greater objectivity, and for large samples the inter-examiner reliability is certainly higher than that obtained in full Rorschach studies; nevertheless, we can expect increasing inaccuracy (and therefore decreasing validity) proportional to the severity of the contraction, so that what is gained in objectivity is more than lost in other, and possibly more important, respects.

Among these short-cut methods, the Munroe Check List has been a particularly popular choice. Designed by Munroe (1945) specifically for the prediction of adjustment and academic performance of college students—in which role it has proved a useful, reliable, and valid psychometric tool—it has since been utilized in a variety of clinical investigations in a context quite disparate from its intended function, and on a completely different sample from that which formed the standardizing population.

Small wonder then, that such investigations produce anomalous, inconsistent, or even contradictory results. Linford Rees and Jones (1951), for example, have shown that in the prognosis for physical treatment in schizophrenia, a number of individual Rorschach determinants were significantly different between the recovered, improved and non-improved groups, whereas on the basis of the Munroe scores, there was no difference, at a statistically significant level, between these groups for physical treatment in general, or for specific treatments such as insulin coma therapy, E.C.T., electronarcosis, or psycho-surgery. This work of Linford Rees and Jones has been useful on two counts, firstly in showing the value of certain Rorschach indices for prognosis in schizophrenia, and secondly for revealing the inability of a check list to discriminate between the grades of improvement.

The authors have felt the need to establish whether or not the Munroe Check List had any validity in psychiatric diagnosis, and chose the three reasonably well-defined categories of normal, neurotic and psychotic states. A sample of 300 was used. The criterion for normality was that the individual had never been a psychiatric patient and showed no anamnestic evidence of psychiatric symptomatology. Most of the normal records (100 in all), were obtained extra-murally. One hundred neurotic and 100 psychotic patients were selected from among the in-patients of a county mental hospital, the diagnosis being confirmed by two consultant psychiatrists. The Rorschach test was administered to each of these patients by the first author, and subsequently scored "blind" by the second author with reference to the Munroe notation. Munroe scores were obtained for each record, and when re-grouped into the correct diagnostic categories, produced the following means and standard deviations: Normal controls 13.1 ± 6 , Neurotics 14.2 ± 7.5 , Psychotics 17.0 ± 7.0 .

These differences, while lying in the expected direction, are insufficiently discriminating at a satisfactory level of statistical significance. From these results it can therefore be concluded that the Munroe Check List is an unsuitable instrument for use in psychiatric personality assessment.

ACKNOWLEDGMENTS

The authors wish to thank Dr. G. E. Duggan-Keen, Medical Superintendent, Winterton Hospital, for making available the case material, and Dr. G. M. Gibb and the late Dr. S. Johnson, consultant psychiatrists, for their invaluable help in diagnostic classification.

REFERENCES

- A.R.N.M.D., *Neurology and Psychiatry in Childhood*, 1954. Baltimore: Williams and Wilkins.
KLOPPER, *et al.*, *Developments in the Rorschach Technique*, 1956. London: Harrap.
MUNROE, R. L., *Applied Psychology Monographs No. 7*, 1945. California: Stanford University Press.
REES, LINFORD and JONES, A. M., *J. Ment. Sci.*, 1951, **97**, 686.

AZACYCLONAL IN MENTAL DEFICIENCY PRACTICE: A PRELIMINARY REPORT

By

WILLIAM B. WRIGHT, L.M.S.S.A.(Lond.)

Senior Medical Officer

The Royal Scottish National Institution, Larbert

SCHIZOPHRENIA occurring in the early years is generally recognized to be a deteriorating illness and one in which therapy has little effect. Tredgold (6) recognized schizophrenia as a cause of oligophrenia and most mental deficiency hospitals have a number of these patients. Although not numerically a large problem, they are among the most difficult patients to handle.

In view of the results reported by Rinaldi *et al.* (4, 5) and Ferguson (2) on the use of Azacyclonal with psychotics in mental hospitals, it seemed desirable to try its effect on mental defectives whose incapacity appeared to be due to a similar if more malignant process. The mode of action of Azacyclonal is unknown but Fabing (1) has shown its ability to block LSD-25 and mescaline induced psychoses. Clinically it has been found to diminish hallucinations and delusions with a resultant improvement in behaviour. Observers also note its freedom from side-effects and compatibility with other drugs such as chlorpromazine, reserpine, and barbiturates.

METHOD

A group of eight patients was selected. From their history and present mental state, they appeared to be suffering from a chronic schizophrenic process which had originated in early years. There had been no remissions in recent years and all attempts to socialize had failed. As the number of this type of patient is small and as each is an individual problem, the use of a control and experimental group was impossible. The patients' behaviour, however, was such that there would be no dubiety if any significant change took place. The staff were well accustomed to the trial of new drugs and had a healthy scepticism of their value. No information was given to the staff as to the likely effect of Azacyclonal. Each patient was given 120 mg. of Azacyclonal per day in addition to any medication he was already receiving. At the end of four weeks, his or her state was evaluated. The patients were not brought together as a group but remained in their usual wards. No change was made in their daily routine.

RESULTS

The results are presented as individual case reports as this is often more illuminating than bald charts and graphs.

Case 1

Female, aet. 43. Admitted 1923. This patient is a medium grade defective who has been subject to frequent episodes of excitement for many years. She expressed delusions of grandeur and appeared to suffer from auditory hallucinations. She would often express paranoid ideas and had made numerous attacks on patients and staff. During treatment with Azacyclonal, her delusions receded and she denied hallucinatory experiences. Her general behaviour improved and she ceased to make assaults on patients and staff.

Case 2

Female, aet. 35. Admitted 1957. A medium grade defective. History of alteration of personality following an operation at 6 years, becoming indrawn and later exhibiting bizarre behaviour. On admission, she was abstracted and took no interest in her surroundings. She sat in a chair muttering to herself and appeared to be hallucinated. She would slap herself and rend her clothes. Treatment with Azacyclonal was followed by more normal behaviour. She ceased to slap herself and she appeared more in touch with her environment. She would converse freely and interest herself in the cinema and television.

Case 3

Female, aet. 14. Admitted 1948. A low grade incontinent patient whose tardy development was noted in early months. Her speech consists mainly of repetitive phrases and she exhibits echolalia. On occasions, she makes remarks which are to the point but this may be coincidental. She is overactive and has frequent episodes of screaming. She is impulsive and bites other patients with such suddenness that they seldom can take avoiding action. Reserpine has been tried but it had to be withdrawn as she became unsteady on her feet. Sodium amylobarbitone has been administered for the past year with only a slight beneficial effect. There was no change in her behaviour with Azacyclonal.

Case 4

Female, aet. 27. Admitted 1944. Medium grade defective with a history of frequent violent episodes for several years in which she would attack patients and staff. Admitted to auditory hallucinations. She frequently exhibited silly inappropriate laughter. Sodium amylobarbitone has been given for several months with a slight quietening effect. Since receiving Azacyclonal, this patient's behaviour has improved. There have been no violent outbursts and she now denies hallucinatory experiences. She still, however, shows silly inappropriate laughter.

Case 5

Female, aet. 40. Admitted 1931. Medium grade defective. History of impulsive behaviour for many years in which she would strike out for no apparent reason. She admitted to auditory hallucinations. On interview, she was tense and hostile. For the past few months, she has been receiving acetylpromazine with a slight beneficial effect. Since administration of Azacyclonal she has made no attacks on other patients and she now denies the presence of auditory hallucinations. Hostility has also diminished and she is now more ready to converse.

Case 6

Female, aet. 35. Admitted 1930. Medium grade defective. Since admission, she has been impulsive in behaviour and has made serious attacks on the staff. Her speech consisted of stereotyped phrases and conversation with her was impossible. She always referred to herself in the third person. She was able, however, to perform simple routine domestic tasks. For the past few months she has been receiving acetylpromazine and since then she has been less tense and there have been no episodes of violence. Following the prescribing of Azacyclonal, she became more communicative and she could be taken outside her usual stereotyped thinking.

Case 7

Male, aet. 18. Admitted 1944. Medium grade indrawn patient who has for several years spent his day sitting on the steps conversing with himself. Admitted to auditory hallucinations. He was liable to strike out for no apparent reason. Since receiving Azacyclonal, he denies hallucinatory experiences and has ceased to strike out. He is also freer in conversation and appears more in touch with reality.

Case 8

Male, aet. 16. Admitted 1951. Medium grade defective. He has been withdrawn and subject to bizarre behaviour for several years. There have been frequent episodes in which he would throw furniture about. Auditory hallucinations were present. He has been on reserpine for the past year and, since then, he has been quieter although he remained hallucinated and withdrawn. Since administration of Azacyclonal, he denies and shows no evidence of hallucinations. He is also more sociable.

No side-effects from administration of Azacyclonal were noted and it proved compatible with sodium amylobarbitone, reserpine and acetylpromazine.

DISCUSSION

Seven of the eight cases benefited from receiving Azacyclonal. Hallucinations and delusions disappeared, aggressive behaviour diminished, and social contact became closer. No attempt was made to socialize the patients as extraneous factors would have come into play. It would seem, however, that intensive attempts to socialize this type of patient, accompanied by Azacyclonal

medication, would be worth while. Ferguson (2) among others has noted the need for psychotherapy to help the patient use his new free psychic energy in a constructive manner.

It is of interest that the one patient who was not helped by the drug was a low grade defective who approximated to what MacGillivray (3) had described as "hyperkinetic larval psychosis of idiocy". Results from one case prove nothing, but there is the suggestion that the aetiology of her psychosis differed from the others in the group.

SUMMARY

Azacyclonal was administered to eight mental defectives who showed evidence of a schizophrenic process which had originated in early years. Seven of the eight patients showed a significant improvement in their mental state.

ACKNOWLEDGMENT

I wish to thank Dr. J. B. Methven, Physician Superintendent, The Royal Scottish National Institution, for permission to undertake this investigation and also for his advice and criticism.

REFERENCES

1. FABING, H. D., *Neurology*, 1955, **5**, 319.
2. FERGUSON, J. T., *Antibiot. Med. Clin. Ther. (Brit. Ed.)*, 1956, **1**, 75.
3. MACGILLIVRAY, R. C., *Amer. J. Ment. Defic.*, 1956, **60**, 570.
4. RINALDI, F., RUDY, L. H., and HIMWICH, H. E., *Amer. J. Psychiat.*, 1955, **112**, 343.
5. *Idem*, *ibid.*, 1956, **112**, 678.
6. TREDGOLD, A. F., *Text Book of Mental Deficiency*, 7th ed., 1947. London: Baillière, Tindal and Cox.

DEMONSTRATION OF LARGACTIL (CHLORPROMAZINE HYDROCHLORIDE) IN THE URINE

By

HANS K. NEVE

Medical Assistant

From Department A, St. Hans Hospital, Roskilde, Denmark

It is often of importance in a psychiatric department to ascertain whether a patient has consumed the Largactil prescribed, e.g. on occasions when a patient appears only to be slightly affected by Largactil. Dubost and Pascal demonstrated that a Largactil solution mixed with equal quantities of concentrated sulphuric acid gives a marked carmine red colour which is so stable that the intensity of the colour may be measured in a spectrophotometer. In biological fluids, Largactil must first be extracted with ether and then transferred to the aqueous phase before the determination can be undertaken. By experiments on rabbits and dogs, these authors found that up to 5 per cent. of the Largactil administered is excreted in the urine in the course of 24 hours and a total of up to 8 per cent. is excreted in the course of the first few days.

A simple method of qualitative demonstration of Largactil in the urine is obtained by employing a reagent consisting of 10 ml. 27 per cent. nitric acid + 1 ml. 2.5 per cent. NaNO_2 . The reaction is carried out by adding 0.5 ml. of this reagent to 5 ml. urine. If the patient has received a large or medium dose of Largactil the urine will assume a blue or blue-violet colour and with large doses this colour is so intense that the solution becomes opaque. The colour is not stable but changes in the course of a few minutes, becoming red-violet, red and later yellowish. If the patient has received only small doses, the colour of the urine becomes only red-violet or red but even quantities as small as 75 mg. daily produce a distinct red colour. Occasionally the colour may, however, disappear so rapidly that its occurrence may be doubted but in such cases the test may be repeated with double quantities of the reagent. Phenergan, which similarly to Largactil is a derivative of phenothiazine, gives the same reaction as Largactil. In the presence of urobilin, light red Largactil reaction must be regarded sceptically, since urobilin with nitric acid assumes a pale pink colour.

The morning urines of 109 patients suffering from various mental disorders were examined with the above-named reagent. Of these, 82 investigations were undertaken without the investigator having any knowledge of the patient or the therapy employed. The patients were distributed thus:

Largactil prescribed	..	56	Largactil or Phenergan demon-	
			strated in urine	 53
Phenergan prescribed	..	2	Largactil not demonstrated in urine	56
Neither drug prescribed		51		
		109		109

The quantity of Largactil prescribed varied from 75 to 1,200 mg. daily and two of the patients received, in addition, reserpine and 5 ritalin.

Among the patients who did not receive Largactil there were two who received 75 mg. phenergan daily. Of the remainder, 9 did not receive any drugs while 22 received reserpine and two of them ritalin, in addition. Other drugs which were administered include Antabuse (5), Restenil (2), Ronicol (2), phenantoin+Luminal (2), Mysoline+Luminal (2), phenantoin+diphenhydramine chloride (1), isonazide (1), PAS (1), insulin (1), penicillin (1) and ferric tartrate (1).

In the latter group, the urine from the two patients who had received phenergan rendered a positive Largactil reaction as anticipated while all the others were negative.

Out of the 56 for whom Largactil had been prescribed, positive Largactil reactions in the urine were obtained in only 51. There were thus 5 patients for whom Largactil had been prescribed but despite this no Largactil was excreted in the urine. The following facts were revealed concerning these patients:

No. 1: 600 mg. Largactil daily had been prescribed for this patient. He refused either to admit or deny having consumed the tablets. In the evening a nurse observed him concealing the tablets in his pocket and when confronted by her he admitted that he had not consumed the tablets prescribed. He was discharged the following day.

No. 2: 600 mg. Largactil daily had been prescribed for this patient. As the Largactil reaction in the urine was negative, the patient was kept under observation and in the evening the tablets of Largactil for the day were found in his clothes.

No. 3: 600 mg. Largactil had been prescribed for this patient daily. Largactil tablets were found in the lavatory but it remained unknown who had put them there. The urines from all the patients in this ward who were receiving Largactil were examined and this patient was the only one whose urine did not contain Largactil. The patient admitted that he had not consumed his tablets and refused to take the drug in any form.

No. 4: 600 mg. Largactil daily had been prescribed for this patient. As the urine showed a negative Largactil reaction, 200 mg. Largactil was administered in solution. The following day the urine gave a strongly positive reaction.

No. 5: 300 mg. Largactil daily had been prescribed for this patient. As the urine gave a negative reaction, 200 mg. Largactil was administered in solution. The following day there was a strongly positive Largactil reaction in the urine.

Finally, 450 mg. Largactil daily had been prescribed for a patient but only traces were found in the urine although a marked blue reaction would have been anticipated. When Largactil was then administered in solution, the urine showed a markedly positive reaction the following day and during the subsequent days the patient himself became markedly affected by Largactil. It must, therefore, be presumed that he did not take the tablets, as a rule, but had swallowed a small quantity "by mistake".

Out of the 56 patients for whom Largactil was prescribed, there were thus 6, viz. 10.7 per cent., who had not consumed the tablets and had thus avoided Largactil treatment.

The substance determined in the urine is not Largactil as such but probably an oxidation product. A solution of pure Largactil always gives a red coloration with the reagent; nor is the blue colour due to the possible influence of substances in the urine as urine to which Largactil has been added also gives a red colour even after the elapse of 24 hours.

Further, the reaction is not due to nitric acid as such but to its content of HNO_2 , and NaNO_2 is added to the reagent to ensure an adequate content of HNO_2 . Freshly prepared 27 per cent. nitric acid produces no or practically no coloration with urine containing Largactil. When set aside, particularly under the influence of light, an increasing quantity of HNO_2 is formed and nitric acid then reacts with Largactil just as the reagent employed, but 6-8 times as much nitric acid must be employed to obtain the maximal intensity of coloration. The fact that the active factor is HNO_2 is also demonstrated by the observation that the same colour reaction is obtained if 25 per cent. sulphuric acid is substituted for nitric acid in the reagent employed. Employed alone, 25 per cent. sulphuric acid does not give any colour reaction with Largactil.

SUMMARY

A simple method of quantitative determination of Largactil in the urine is described. This reaction depends upon a specific colour reaction which takes place with nitric acid + sodium nitrite. Employing this reaction, it was revealed that approximately 10 per cent. of the patients for whom Largactil was prescribed did not consume the tablets.

ADDENDUM

It may be mentioned that investigations are at present being undertaken regarding the period of excretion, viz., the period from the withdrawal of medication until the urine is free from Largactil. Preliminarily, a period of excretion varying from 4 to 12 days has been found and this variation appears not to depend solely upon the quantity of Largactil consumed.

REFERENCE

DUBOST, P., and PASCAL, S. *Ann. pharmaceut. franç.*, 1953, **11**, 615.

JOINT MEETING
OF THE
ROYAL SOCIETY OF MEDICINE (SECTION OF PSYCHIATRY)
AND THE
ROYAL MEDICO-PSYCHOLOGICAL ASSOCIATION
(MAUDSLEY BEQUEST)
ANGLO-AMERICAN SYMPOSIUM

held at
The Royal Society of Medicine, 1 Wimpole Street, London, W.1
on
Tuesday and Wednesday, 10 and 11 September, 1957

First Day: Tuesday, 10 September, 1957:

Chairman: Dr. R. W. Armstrong
(President, Royal Medico-Psychological Association)

"The Teaching of Psychiatry"

DR. EWEN CAMERON.

Discussor: Dr. Noel Harris.

"The Teaching of Psychotherapy"

DR. JACOB FINESINGER.

Discussor: Dr. Denis Leigh.

"The Neurotic Process"

DR. LAWRENCE KUBIE.

Discussor: Dr. Emanuel Miller.

"The Treatment of Depression"

DR. OSKAR DIETHELM.

Discussor: Prof. E. Stengel.

Second Day: Wednesday, 11 September, 1957:

Chairman: Dr. William Sargant
(President, Section of Psychiatry, Royal Society of Medicine)

"The Use of Genetics in Psychiatry"

DR. FRANZ KALLMANN.

Discussor: Dr. Eliot Slater.

"Physical Methods of Treatment in Psychiatry"

DR. LOTHAR KALINOWSKI.

Discussor: Dr. L. C. Cook.

"The Use of Tranquillizers in American Psychiatry"

DR. PAUL HOCH.

Discussor: Dr. Maurice Partridge.

"Biochemistry and Schizophrenia"

DR. HOWARD FABING.

Discussor: Dr. D. Stafford-Clark.

THE TEACHING OF PSYCHIATRY

By

D. EWEN CAMERON, M.D.

Department of Psychiatry, McGill University, Montreal

THE preparation of the men to staff the psychiatric centres and services, the laboratories and installations which have come into existence in such numbers and within so short a period in almost every country has been a task of the first magnitude.

All major universities on the North American continent have now opened up departments of psychiatry and all others are clearly destined to follow their leadership. Clinical departments have been and are being opened up in countless associated general teaching hospitals. Clinics and laboratories and instructional centres of all kinds have been organized to provide not only the basic teaching but also the increasingly diversified instruction which is now being demanded as our field becomes the more variously structured.

Since World War I on the North American continent alone more than 10,000 psychiatrists have been trained and many times that number of men preparing themselves to enter other fields of medicine have been given a knowledge of human behaviour entirely unavailable to their predecessors. This has been one of the great academic enterprises of our times.

Sweeping though these administrative and organizational designs have been, we can see the nature of this massive movement in most living form in the new premises of psychiatric teaching.

The teaching of psychiatry has been developed under circumstances of pressure and stress which are exceptional. On the one hand we are impelled by a demand of the utmost urgency that medical men must immediately be prepared and fitted to deal with mental illness, so costly in material terms, so devastating of human nature. They are exceptional because of the now widespread professional and public recognition of the immense and pervasive penetration of mental illness throughout all our populations. On the other hand the fact that the development of psychiatry has been so long delayed is due not to simple neglect but arises from powerful, as yet ill-understood, opposition to facing this area of human life and health.

This opposition comes from sources deep within our system of social beliefs and organization. It comes from struggles with our instinctive drives, from our attempts to control them by deprecation and banishment. It comes from the singularly though narrowly successful development of our system of medicine, based upon a philosophy of science derived from the physical disciplines of biochemistry, physics and chemistry—a system which is ill-fitted to deal with those non-logical, non-rational, uniquely personal factors which we now know so greatly to affect the course of human ill health.

The outcome of this long struggle between these contending forces is being swung in favour of humanism. For within the last fifty years there has arisen into dominance in all our societies the determination that we must have and put to work a knowledge of human nature far more effective than we have yet possessed.

This demand springs from great and compelling sources. The exceptional expansion in numbers, power and complexity of human societies everywhere, require as a simple necessity of survival that men understand themselves and each other to a degree which they have not in the past. Moreover as our societies grow in power and complexity we have a need growing in equal pace to provide men fit to operate these societies, and hence, wherever we turn, whether it is to industry, to education or to the expanding fields of communication or to medicine itself, we find this insistent demand that those who are preparing themselves to enter these great and central areas in the societies of men must be fully versed in our modern knowledge of human nature.

In this presentation I shall not deal with the technical procedures, interesting though many of them are, which have been developed to aid in the teaching of psychiatry. Nor shall I deal with the great administrative problems, such as how to ensure that the number of men trained in psychiatry may rapidly be made adequate to meet the overwhelming demands that are now swamping even the reinforced numbers of us already in the field. Rather I shall endeavour to place before you certain new conceptions concerning the teaching of psychiatry which have been taking form in the last several decades and which are now being set into action. It is perhaps of particular interest that these new conceptions finding their primary origin within the thinking of the teachers of psychiatry are now none the less spreading, as we hoped they would, to other disciplines of medicine.

Hitherto it has been customary to think of medical teaching as a pedagogic procedure which finds its methodology in a traditional system of pre-clinical and bedside teaching, seminars and clerkships, all expressed in the organizational form of a curriculum to which the various departments make their often highly separate contributions. In this discussion I am proposing that we should look at the preparation of men, both at the undergraduate and postgraduate levels, in terms particularly familiar to us, namely, as a psychosociological process bringing about changes in these men—changes in their knowledge, their skills and their attitudes, these being the changes which we consider necessary in the preparation of the medical man and later the psychiatrist.

We are presently much concerned to see to it that our teaching is no longer based upon those abstractions, diseases, but is founded upon the living realities of people who are sick. There is every sign that this self-same intense concern with individual human nature is leading us to see the student and to work with him also as a person. In a word there is a shift from the teaching of classes to the teaching of individuals.

In a former day the student was seen as a mass unit rather than as a person—much as we conceived of the economic man, the typical boy, or the average housewife. The undergraduate student, it was believed, came to his first Monday morning lecture innocent of all beliefs and information likely to affect his taking in the medical knowledge with which he was to be inculcated. Thus it was thought that by suitable exposure to lectures, by watching how his instructors worked, by reading to amplify and engrave what had been taught him, there ultimately became laid down in his mind the myriad pin-points of facts, the patterns of the required skills, and the network of theories necessary to bind the whole into the standard form of the well-trained professional man.

The process, as can be seen, owed much to the model of the machine which then dominated our thinking concerning how the human being lived and worked.

Now this has changed. It has changed in considerable measure by direct

studies of the learning process, and to this psychiatrists have made their contributions through their investigations of individual and group dynamics. We now see learning as a process which brings about change. It is a process which achieves change in the individual in the form of increased knowledge, increased capacity to recognize and understand health and illness; change in the form of the acquisition of a complex series of skills, the development of the essential scientific, professional and humanistic attitudes. These changes go forward under the incitement of varying motivations and through group and individual reactions.

The group reactions take place between the instructor and the student, the student and the patient and between the students themselves. The learning process also proceeds in terms of intrapersonal procedures. This perhaps we are in some danger of forgetting in our intense concern with group dynamics. None the less reflection and reading, conjecture and solitary consideration can and do lay the basis for some of the more solid learning which we achieve. We see, then, in the terms of our new conception, that learning is a process, and that our knowledge of this process and of what alteration it is necessary to bring about through that process represents the central core of our medical teaching in general, and in particular our teaching of psychiatry in the undergraduate and postgraduate curriculum. This is basic to our understanding of the developments which are going forward in medical education. It forces us to rearrange many of our conceptions of teaching; raising some into prominence, abolishing others and indeed bringing some altogether new dimensions of teaching into consideration.

Knowledge, as we are aware, does not expand in one direction only but spreads out, unevenly it may be, but certainly in all possible directions. Hence we have come to recognize that learning is not only a process but also that it is far from being a simple two or three factor interaction: student/instructor/material. The old saying that the best university is a log with a student sitting on one end and Dan Webster on the other is, at the most, picturesque. We are increasingly aware that a great array of factors promotes or hinders the process of learning. To investigate and understand the highly complex and powerful forces which act upon the medical student and the psychiatrist in training, to study how they promote or hinder the appearance in him of the desired changes, we may with great advantage make use of the working concept of the field of force.

Using this concept, we see the student on coming to the university as entering a field of social force, this field being comprised of the medical school, the hospitals, the clinics and community services, the places where he meets his fellow students or where he thinks and studies by himself, his own capacities and attributes and his various instructors. This is the field in which those forces act which will ultimately turn him into a doctor and later into a psychiatrist.

This concept of the learning area as a field of social force has a number of values. In the first instance it brings out clearly the fact that learning is affected by a multiplicity of factors. As we study each component of the field we see for instance that the size of a group in which the student studies is of importance, as is the psychosociological structure of a particular clinic, or ward or service in which he works. And we are realizing that even the architectural structure and seating arrangements may have an effect on learning.

The second value is that it breaks us free from the long set, firmly binding conceptions as to how teaching should be carried on, conceptions which have been taken over somewhat uncritically by the numerous newly established

departments of psychiatry. And finally it is of particular value in demonstrating the vast amount of exploration and experimentation which awaits us before we can expect to have more than a beginning knowledge of the processes whereby learning goes forward.

Let us look first at the most important component of the field, namely, the student and then at some of the relations which exist between him and other constituents.

We now know that on the very first day that he comes to medical school he already has definite attitudes and basic premises concerning human nature and that these greatly affect his capacity to learn. Moreover, the student, far from being a unit is as various in his nature as the sands of the sea. None the less we may say that it is true of the majority who come that they have a warm interest in human nature.

Here we must record a most disturbing fact, namely, that the first-year student comes with a mind more open to perceive the facts of human nature than he will have after he has completed the first year or two of his medical curriculum. For during this year or two he will have been much impressed by, and strongly indoctrinated with, the great values of the use in medicine of a scientific method which unfortunately was evolved not from the study of living organisms but from our investigations of physics and chemistry, astronomy and mathematics.

One of the major advances in our understanding of teaching in the undergraduate years has been our recognition that along with the vast gains made through the application of the scientific method to medicine there have also sprung up some serious consequences. These consequences are first the appearance in our teaching of the principle, widely used in experimentation, of the isolation of the subject under consideration from its context. Hence we have subjects pursued in fragmented isolation from the total person as has been the case in some of our teaching of pathology, of biochemistry, and of physiology, or of patients immured in hospitals and studied in isolation from their social settings. We have also noted a tendency to omit from consideration what cannot easily be made to fit into the framework of the experiment. That only too often has been the most highly adaptive and therefore the most variable aspect of the whole functioning of the individual, namely, some areas of his adaptive and instinctive behaviour.

Let us turn from the attributes of the student to the interactions which go on between him and the other components of the learning field which he has entered.

About these interactions we have some information. We can certainly say that in the last four or five decades we have come to understand that the small group constitutes a most important learning area. We can also say from our growing knowledge of group dynamics that the amount of learning which goes on in small group instruction has a positive relationship to the amount of individual participation, maximal learning tending to take place at the point of the asking of a question. Hence both in the undergraduate and in the post-graduate teaching of psychiatry great and growing weight is placed on small group instruction with maximal student participation.

As we watch the processes of learning going forward within the field of social force which we have described we can discern a second great area of relationship in which learning reaches levels of particular intensity. This area is constituted by the relationships of the patient and the student, whether undergraduate or postgraduate.

Because of our appreciation of the importance of this relationship we have already set up, both in our undergraduate and postgraduate curricula, measures to take advantage of its special value as a means of learning. In the undergraduate curriculum these measures are still new and are still in the process of being worked out. The first is quite simple. In many medical schools the student is brought in contact with the patient as early as in his first year. This has great advantages in so far that this early contact, which is continued, of course, throughout the whole four years, offsets the trend towards fragmentation of the patient and pre-occupation on the part of the student with systems—cardio-vascular, skeletal, and gastro-intestinal—rather than with the living person. It has clear disadvantages since the student at this stage in his career has passed through only the opening phases in his instruction in pre-clinical subjects and can have little more than a layman's grasp of the clinical problems set before him.

In a few centres this early contact with the patient is structured more completely and the student for instance is assigned as an observer to a woman in the sixth month of her pregnancy. He follows her not only throughout the whole course of her pregnancy but throughout his curriculum. He keeps in touch with the family and watches the growth and development of the child as well as the relationships which exist between the child and the other members of the family.

In other centres the student may be assigned to an already established family. He acts as a medical friend during the whole course of his curriculum. This arrangement, attractive though it may seem theoretically, has certain practical difficulties, among them being the obvious one that the medical school sooner or later finds itself responsible in some measure for the well-being of some 400 or more families comprising perhaps 1,500 to 2,000 people.

The clinical clerkship, of course, is a procedure long established in other disciplines of medicine. With the coming of psychiatric divisions of general hospitals the clerkship has become an increasingly common and valuable asset of the undergraduate training of the future medical man.

When we come to the postgraduate training of the psychiatrist, the learning which goes forward in the patient-therapist relationship is, of course, of outstanding significance and hardly requires to be documented in this communication. We may, however, point to an interesting phenomenon, namely, that in the last several decades it has been found increasingly valuable to introduce a third person into this relationship.

This third person is the tutor. To this senior member of the department the postgraduate student goes perhaps twice a week with the problems he has been encountering in his work with the patient. We first introduced this procedure in our centre because of the considerable anxiety which was apparently experienced by the young man in training when facing some of the psychodynamic and psychotherapeutic problems which came before him. We have subsequently found moreover that in addition to alleviating his anxiety it greatly promotes his learning. At the same time it throws light on another forward step to the understanding of the learning process. We had occasion earlier in this paper to stress the fact that learning is not a simple matter of recording but does involve considerable changes, changes which take place for the most part as a consequence of interactions going on between the student and the patient, the student and his instructors and the student and other students. This comes out particularly clearly in the tutorial situation and advantage is taken of this to show the student the importance which factors in his own

personality have for his understanding of his patient and for the management of the case. His hostilities, his blindspots, his anxieties, all can be shown in operation in this relationship and all can be dealt with through the unique position held by the tutor.

Turning again to the learning process as it goes forward in the field of social force, we can point to other areas in which the process goes on with maximal intensity. We have already referred to the learning which goes on when the student is studying by himself and reflecting and considering what he has learned. There is some reason to feel that some of the most important learning goes on in student discussions. These various situations have been mentioned primarily for the sake of completeness. One must be impressed however with the limited amount of knowledge which we have of these extremely important areas. We must be no less impressed with our willingness hitherto to continue to construct our curricula largely on the basis of traditional beliefs concerning learning.

Let us now turn from the relationships which exist between the various components in our field of social force and consider what changes we seek to bring about in our students, both undergraduate and postgraduate. For convenience rather than for exactitude we may divide these changes into changes in attitude, acquisition of skills and the accumulation of factual knowledge.

Both curricula have come under the sharpest possible scrutiny in post-war years. The First World Conference on Medical Education held in London in 1953 marked a high point in the intensity of this scrutiny. Since that time in many countries, national, regional and individual university bodies have been set up to study undergraduate medical teaching.

The content and purpose of both the undergraduate and postgraduate curricula has been intensely reviewed.

Hitherto we have tended to look upon the medical curriculum, both undergraduate and postgraduate, as something developed entirely within the field of medicine, distilled from long tradition, conservatively modified by departmental and faculty discussions. Yet in very fact the curriculum is a social structure which most assuredly does not exist in isolation from the world and times which it serves. It is true that the immense growth of medical knowledge has been a crucial factor in requiring the present scrutiny of the curricula but we should fail to grasp what is going forward if we did not realize that it is the expectations of our national communities with regard to health and in a broader sense with regard to the enjoyment of life that are exercising the most powerful effects on our preparation of the basic doctor and the specialized psychiatrist.

We have undoubtedly come to the end of a period in which men were trained, in the undergraduate curriculum and postgraduate courses, for diagnosis and treatment, for the detection of disease and its conquest. We have entered insensibly into a period where the primary concern is the individual and also the community.

It is in the attitudes of the new basic doctor and the new psychiatrist that we can most easily distinguish the impact of the expectations of our communities.

This latter consideration has now come into the foreground. Indeed it is the necessity of creating new attitudes, of forming new kinds of orientation towards health and illness more than the accumulation, great though it is, of new knowledge or skills which is bringing about the revision of our curricula.

In earlier days attitudes were something picked up more or less insensibly from the traditions of the school in which the young man studied and from the

example set by his teachers. Now we are drawing up explicit plans to foster the attitudes which our days and the practice of psychiatry require and in a broader sense are attempting to expand this to the whole field of medical education.

The first major attitude consists in a shift from concern with disease to concern with the patient, a shift from preoccupation with disease to preoccupation with health. This is beginning to show itself in psychiatric teaching in the undergraduate curriculum and in the fact that we are now to a far greater extent than before concerned, not so much with the teaching of junior psychiatry but with the teaching of the facts of human nature.

A second great attitude is one to which we have not given nearly so successful expression and that is that one must look at the whole person.

Despite panel teaching, the integration of teaching of groups of departments and the development of the psychosomatic approach we are still inclined to think in terms of functional and organic. The teaching of psychiatry in the undergraduate curriculum must clearly transcend the borders of the department. Unless some of our basic concepts are taken up by other departments and used by them we shall have failed. Thus far the number of other departments which carry out a complete examination of the patient on admission is quite small and certainly fewer still teach this to their students.

With regard to the gaining of skills and the accumulation of factual information a most interesting development is apparent. This is a growing differentiation between the kind of skills and the sort of facts which are being taught in the undergraduate and postgraduate curricula. At an earlier day the teaching in the undergraduate curriculum was a modified version of our postgraduate instruction. We taught undergraduate students junior psychiatry.

Now with our growing acquaintance with the enormous amount of psychosomatic illness, with the extent to which emotional and other factors complicate all types of illness, we are beginning to give more precision and a special identity to the instruction which we give at the undergraduate level. A course of descriptive psychiatry, of necessity, is included, since it is essential that the major types of psychiatric disturbance should be recognized by the future doctor, but in so far as we see the future doctor applying psychiatric concepts and working to an increasing degree with the human factor in illness, just as he does with bacteriological agents, we now seek to prepare him. We present to him the human factor in illness, the management of the patient, the doctor-patient relationship with all its potentialities for health and all its potentialities for damage. We seek to acquaint him with the nature and the dynamics of the interview and with the basic facts of human growth and development, with the ways in which childbirth and puberty, marriage, the menopause and bereavement are managed. We put before him our knowledge of our sexual drives, of our hostilities and our anxieties and how they may affect our health.

The means whereby we do this are also changing. One-way screen instruction, the use of the teaching movie and of the clinical clerkship are steadily growing.

At the postgraduate level the amount and the range of information and of skills which the student has to learn is increasing with great rapidity. There is no doubt that we are again approaching in the post-graduate curriculum the dilemma which we have long encountered in the undergraduate curriculum, namely, the impossibility, because of the vast speed of accession of knowledge, of ensuring that all the necessary data can be put before the student. We are falling back as we did in the undergraduate curriculum upon an attempt to define what areas are fundamentally important.

The matter of attitudes has already been dealt with in our earlier discussion on the undergraduate curriculum. These attitudes, which call for concern with the person rather than with the disease and with the whole person rather than with a fragment or system, are no less valid in postgraduate training. Only one other major attitude will be dealt with in this consideration of the postgraduate curriculum. It is born of the very difficulty and complexity of our field. The range of variables affecting any aspect of behaviour is so considerable and the data in itself so highly abstract that in psychiatry to an extent probably found nowhere else in medicine the postgraduate student is apt to feel lost, bewildered and insecure. For this reason there has been a tendency for the teacher to be over emphatic and for the founders of schools—it is one of the happier circumstances of the growth of our discipline that the founding of schools is now largely a matter of the past—to be quite iconoclastic in their exclusiveness. For the insecure student this is a refuge. Yet at the same time exclusive devotion to a viewpoint, while possibly affording some relief from anxiety and giving some direction to those students who cannot find their own way, is a most crippling and disabling procedure.

With our growing knowledge of how attitudes may be inculcated, beliefs manipulated, there is a serious obligation upon us to be constantly alive to, and to take every means of avoiding, the dangers of discipleship.

Turning now more directly to those areas of skills and knowledge which are showing the most rapid development, we must now stress something which in the last several years we have been in some danger of neglecting, namely, the great desirability of making phenomenology a basic area for learning in the postgraduate curriculum.

Dynamics has had a remarkable expansion and we owe much of the success of modern psychiatry to the growth of our understanding of human dynamics. And yet it is ultimately from phenomenology that some of the most important new lines of development take their origin. It is by returning continually to the basic facts of observation of what is happening, of how it can be modified, that we may hope that new fields may be opened up and answers may be found to problems which are very old indeed. Hence we find in many centres a growing utilization of the newer tools of recording, of movies, of magnetic tape recorders, one-way screens, the electromyograph, the electrogastrograph and the galvanic skin reflex, as a means of re-examining under the microscope of such tools long known behavioural disturbances such as psychomotor retardation, excitement and the functioning of those mechanisms which lie between stress and symptom.

Several of these tools have also permitted a most considerable expansion in our understanding of dynamics and psychotherapy. We ourselves supply all our postgraduate students with a tape recorder in much the same way as a student in pathology is provided with a microscope.

As a first step to successful work in psychodynamics or psychotherapy, whether individual or group, must come a prolonged training in problem identification. The recorder we have found to be quite invaluable as a means of detecting problems as shown in the patient's communication and of showing defects or strong points in the therapeutic participation of the student.

Every day with every mail we are reminded of psychopharmacology. This field is so earnestly debated in conferences and individual papers that it is barely necessary to do more than indicate that it seems probable that it is destined to be vastly expanded. We may however in passing point to two great steps which must be taken before it can play its proper part both in the preparation of the student and in the ultimate treatment of the patient. The first

is the setting up of adequate methods for experimental investigation of new psychopharmacological agents, and the second is in the provision of a theoretic structure which would permit the systematic development of new agents to meet specific needs. At the present time it is almost incredible by what rule-of-thumb procedures pharmacologists working in the drug houses apparently proceed to find new agents.

The area of the physical therapies in psychiatry is rapidly growing so complex in itself, requiring such knowledge of among other things electronics, as to show some signs of becoming a sub-speciality in itself.

This brings us to a serious consideration, namely, the emergence of special fields of work. We can note immediately that child psychiatry, community psychiatry, and industrial psychiatry have already been designated as separate, at least by name. Administrative psychiatry has also been designated and we know well that many men devote themselves almost exclusively to research and others to consultation work, and others to various forms of psychotherapy. We also know that any one of these fields is a lifelong study. To meet this therefore we ourselves have revised our four-year programme and now devote the first two years to a basic training course through which all of our post-graduate students must pass. In the last two years choice is offered and those individuals who are going into child psychiatry will spend those years in that subject, in community psychiatry and paediatrics. Those going into research will clearly spend an increasing amount of their time working under direction in the laboratories and those going into other sub-speciality fields already mentioned will receive their training accordingly.

In any ongoing process whether it is biochemical, psychological, or sociological, we are of necessity interested in one most important attribute, namely, the intensity with which the process is going forward. Hence we must be concerned with respect to the learning of psychiatry both in the undergraduate and postgraduate curriculum with the intensity with which changes are going forward within our field of social force. This is something which in the past we have dealt with only to a limited degree and in terms of traditional concepts of how learning can be facilitated.

Motivation has been sought through the use of examinations. But this is a crude and faulty arrangement and there lurks the ever-present danger that the student will prepare himself to pass the examination rather than to be a psychiatrist.

It is certain that the coming years must find other motivations and it is no less certain that the psychiatrists among others will be consulted as to how this is to be brought about. As we survey the situation we may say that we recognize, perhaps more clearly than some of our colleagues, that learning has an emotional component and that answers to our problem are probably to be found in studies in depth of those students who seem to be strongly motivated as contrasted to those perhaps otherwise no less intellectually gifted who have little motivation and may fail. Even now we can point to the fact that participation usually increases motivation, hence one of the values of small group discussion learning. We may also point to the fact that increased responsibility for the care of patients is a strong and important motivating factor.

We now pass to the last but assuredly no less essential aspect of the operation of our field of social force, and that is the assessment of what has been achieved. References have already been made to the use of the examination and to its limits. We are all much aware of what a weak thing it is, since a man who may do well in examinations may be the poorest of physicians. We are also

aware how little examinations can show of those qualities and attitudes which are essential in a physician. Some of these qualities are well known but there are others of them which become apparent to us when we meet and live with our fellow physicians. We see that patients get well, or don't, or die and we see that in some measure this is due to the extent to which the physician has a tireless persistence in their care, that he has resiliency to rebound from failure, that he has courage to take some step for the welfare of his patient, a step where if he fails the results may be damaging to him. For often he is faced with the choice of taking such steps or continuing in relative inaction for which he will not be blamed. Few of these imponderable but vitally important attributes can be discovered by examination. They can however be discovered by those who work and live with the student during his undergraduate and postgraduate training. Hence as we increase our emphasis upon small group discussion, as we employ tutors to a growing degree, as instructors themselves gain more knowledge of human nature and with the assessment of attitudes and emotional factors, we may anticipate that our capacity to measure what has been gained will be increased. And I have no doubt that our efforts in this regard will be supplemented by psychological tests still to be devised.

In this presentation we have sought to look at the preparation of the physician and later of the psychiatrist not as being brought about by the operation of traditional pedagogic principles but rather as a procedure whereby men enter an intricately structured field of social force and there participate in exchanges designed to prepare them for service in one of the great key areas of our societies.

It is a provocative thing that the psychiatrist, keenly interested though he is in teaching and research, when he approaches the matter of teaching neglects much of what is known of both fields. It is the rarest of centres that is at pains to instruct its potential teachers in the methods of teaching and certainly still rarer is it to find that the learning process in the undergraduate or postgraduate curriculum has been studied by research methods. This conception of learning as a process taking place in a field of social force has been put before you in the conviction that it is from psychiatry that there must come the impetus to initiate research into the processes whereby the student becomes the physician and reaches the breadth and stature of the psychiatrist.

DISCUSSION

By Dr. Noel Harris

I take it that the role of a "Discussor" is two-fold. Firstly, to criticize the paper that has been read, and secondly to promote discussion.

I find myself in difficulty over the first point, as I agree with so much of Dr. Cameron's paper, and I should at once like to pay my tribute to him, and congratulate him on it.

I cannot help thinking from what he has told us that in the North American continent there has been a quicker and greater development in teaching psychiatry than in Great Britain. Over here, in some Universities and Hospitals, there is still considerable reluctance and in some cases opposition to making use of the increasing knowledge of psychology. A great deal more could still be done in linking up psychology and psychiatry with all branches of medicine. It is still not uncommon in Great Britain to hear criticism, some of which may be justified, of the attitude of many medical men on "the other side of the

Atlantic" towards psychology, analysis and tranquillizers, but I feel some of the criticism is due to prejudice.

I think that public opinion and interest concerning mental illness is being aroused more slowly in Great Britain than in Canada and America.

I do so agree with what Dr. Cameron says about the importance of working in the closest co-operation with other members of the medical profession. I shall mostly reserve my remarks to undergraduate teaching on which I have been engaged for about 26 years. I consider that it is most important for medical students, nurses and all those training for the ancillary services to be taught how to establish the best possible personal relationship with their patients, and also the importance of the emotional factor in dealing with illness. I feel strongly that it is the physician, surgeon or gynaecologist who should teach these facts more frequently and as a routine, *pari-passu* with the pathology, diagnosis and treatment. I hope that the day may soon come when at my own hospital and all other teaching hospitals, the surgeon who is doing a mastectomy for instance will teach the students about the emotional factors involved in such an operation, and that the gynaecologist will do the same when performing a hysterectomy.

The need to consider the social side in health and illness has I think been well taught in Great Britain when we were well in the forefront with almoners. It is the understanding of the unconscious motivation of patients and the deeper reasons for their reaction to illness which is so often not taught at all. I also agree with Dr. Cameron on the importance of teaching more about health. We should pay more attention to prophylaxis and remember to promote health. Such a venture as suggested by Dr. E. F. Griffiths, whose books many of you must have read, namely, what he has called "An Institute of Family Relationships" would be well worth while in promoting health. Before the war, at the Middlesex Hospital, Professor Moncrieff and I had occasional evening groups which the parents and children who were attending the Infant Welfare Centre and Professor Moncrieff's out-patients could attend to discuss with us not only the physical health of their children but also their character formation.

I agree too with Dr. Cameron about the importance of establishing the best possible relationship between those teaching students and the students themselves. It may be interesting to note that recently the Dean of the Middlesex Hospital has started a scheme whereby each member of the consulting staff has a small group of students attached to him or her, so to speak, for providing those students with some social life and help and guidance if they want it.

I was interested in the idea of the early introduction of the student to the patient and would like more information as to the results.

I have been giving lectures in psychology to students doing anatomy and physiology since about 1936 and I am sure that this is important, but the lectures should be given not too near the time of the examination when naturally the students' thoughts are chiefly concerned with getting through their examinations. Then there are lectures and demonstrations on applied psychology in the clinical introductory course, and, about the third year, lectures and demonstrations on clinical psychiatry are given and all students attend the department of psychological medicine for a period of three months during which time they can see both out-patients and in-patients.

We seem to be faced with the problem that our present method of teaching for examinations is not really the best form of education, and one question I would like to ask Dr. Cameron is if he considers questions on psychology and

psychiatry should be set in the ordinary medical examinations. Time is running on, but one further question I should like to ask Dr. Cameron is about the use of a recording instrument. I have always been rather against having a recording instrument during the examination of a patient because of the possible distress to the patient and the question of professional secrecy. Has any difficulty been experienced over this?

THE TEACHING OF PSYCHOTHERAPY

By

JACOB E. FINESINGER, M.D.

Psychiatric Institute, University of Maryland, Baltimore

FOR the past twenty years we have been studying methods of teaching and their effectiveness in changing the behaviour of medical students and doctors. At the Massachusetts General Hospital our work was concerned primarily with teaching psychotherapy to medical students, residents and younger physicians. During these years we have practised and taught many kinds of psychotherapy, including situational therapy, relationship therapy such as catharsis, support, reassurance, suggestion and persuasion, and the various types of insight therapy from the verbalization and understanding of simple correlations to more extensive Freudian psychoanalysis. The patients we treated in Boston were primarily those with psychoneuroses and psychosomatic disturbances. In these groups of patients insight therapy was the method of choice. It was only when insight therapy was impracticable or too disturbing to the patient that other methods such as relationship therapy were used. During these years we used insight therapy whenever possible, often with limited effectiveness. The treatment for any given hospital admission required from 20 to 40 interviews, occasionally more. It was our practice to use the vis-à-vis interview method.

Since we are to discuss the teaching of psychotherapy I should like to describe in a little greater detail the type of therapy we were trying to teach. Obviously we were interested in teaching methods of diagnosis and of formulating the psychodynamics in our patients—but our major emphasis was on procedures of interviewing that we adapted and found useful in insight therapy (1).

This form of insight therapy owes much to psychoanalysis, both in scope and in technical procedures. As in psychoanalysis, the therapy relies on an effective doctor-patient relationship for the production of material for interpretation and assimilation. The material is made up of behaviour, talk, intonation, gestures and feelings; it includes the whole gamut of verbal and non-verbal reactions. The ultimate goal of insight therapy that we practise may be limited to a less fundamental rearrangement and enhancement of the personality than in psychoanalysis. When used in relatively brief psychotherapy, it seldom exposes the phantasies and memories that are most deeply repressed. As a rule, transference material and dream material are not used to the extent that they are in psychoanalysis. But there is nothing to prevent goal-directed procedures from being applied in a longer and more extensive treatment. And indeed, we have so applied them. In such cases, the patterns of the patient are followed in greater detail through the use of dream and transference material, relying constantly on the fundamental principles that we find helpful in insight therapy. These principles are the development and utilization of an effective doctor-patient relation, the use of goal-directed planning and management, the focusing of material, and the use of minimal activity.

It is our experience that these procedures can be taught and that students can learn to apply them starting with their first patient. The teaching method that we find most effective consists of reading verbatim interviews of a current case in which the remarks of the doctor as well as the patient are recorded. In these supervisory conferences the methods of interviewing and management are formulated and discussed in reference to the situation as it is presented. Dynamic formulations of the patient's productions are elaborated, but the emphasis is on procedures—what to do. Teaching is most productive when the students actively participate in these discussions. The student learns that the very elements in his own behaviour which make for success in the conventional social situation may be precisely contra-indicated in psychiatric interviews used for insight therapy.

The four guiding principles that we have found useful in insight therapy have been referred to above. The enumeration of specific principles in a field as fluid as psychotherapy may seem arbitrary and even challenging, but it is our impression that experienced therapists will recognize much in common between these principles and the rationale of their own working procedures. These working guides could be useful for other forms of psychotherapy. The extent to which such guides would include these four principles can be determined only by study and evaluation.

In collaboration with Dr. Florence Powdermaker these principles have been presented in a series of films on psychotherapeutic interviewing (2), which I am sure many of you have seen.

The method of teaching was as follows: when a new patient was admitted to the service for treatment he was assigned to an assistant resident or to a fourth year medical student. After a brief history had been taken, the physical and laboratory examinations and a mental status examination had been completed, the resident would meet with the instructor individually or with a small group of residents and students. A tentative diagnosis was made, the type of therapy was decided upon and immediate and ultimate goals were set up for the treatment. Usually the immediate goal in the area of material was a detailed description of the current symptoms or problem. The purpose of this was to enable the patient to describe in detail his symptoms and the setting in which they occurred. The resident was urged to write down the verbatim remarks of the patient, to write down his own comments as the interview proceeded—as accurately as possible. At the next supervisory session, which was preferably the next day, the resident would present his material. He would state his goal and read the verbatim interview, being sure to include his own remarks as well as those of the patient.

The interview is usually first scrutinized as interaction material; the resident's interventions and the patient's material are discussed in detail. The questions dealt primarily with the appropriateness of the resident's interventions and their effectiveness in reaching his stated goal. The inexperienced therapist is usually unable to set up meaningful goals; he also has difficulty in proceeding systematically toward achieving his goals even if he can state them. He is usually not clear about his goals, follows the patient's leads and for many reasons—usually unbeknown to himself—goes off at a tangent. The discussion of this aspect of the interview points out just how the patient's productions are influenced by the behaviour and interventions of the therapist.

After a few interchanges have been discussed and have been appraised in terms of goal achievement the material is reconsidered in detail as to its content. The resident and the group are asked to scrutinize the interview material to

note the sequences of associations, to determine whether it is possible to see trends and patterns in the verbal material—and whatever non-verbal material is available. Once such a trend or pattern is unearthed an attempt is made to determine whether this pattern repeats itself in the material and whether it is in any way similar to patterns seen or inferred from the material of the patient as obtained in the history. As the resident acquires more skill and technical facility he is able more readily to set up reasonable goals, to become more skilful in working towards them, and to become more flexible in changing goals. He becomes more sensitive in appraising the subtleties of the patient's material and his own role as a participant in the doctor-patient interaction. During the early stages of the work with a given resident the entire supervisory session may be spent in discussing two or three interventions, and in setting up the goals of the next interview. After several supervisory periods it is possible to cover an entire interview or the major part of an interview in a single session. Later on usually less time is spent on technical matters relating to interviewing and more time becomes available for understanding the content and meaning of the material. As the resident becomes more experienced it becomes possible to discuss every other or every third interview. In the supervisory meeting the instructor encourages the resident to bring up his own questions and problems. The purpose of the discussion is to solve the problems. The role of the instructor is to keep the discussion alive—to point it up if necessary and to do this with the minimum amount of talk on his part—in order to stimulate the maximum discussion on the part of the group.

The advantage of group supervision is that many students can learn from the work of one student. We have also found it practical and even wise to supervise a mixed group of students—two or three medical students, two or three first-year residents and two or three more advanced residents. Since the technical methods are the same and are consistent it is possible for the more experienced students to teach the less experienced students. The more advanced doctor may be reporting on some earlier material—let us say, he is correlating a childhood experience with his patient's current problem; the less experienced therapist may then present material in the current experience of his patient. The medical student may present his problem of getting the patient to communicate the details of his patient's current illness. The methods of creating a therapeutic setting, the methods of helping the patient talk about his current problem, the methods of helping the patient struggle with earlier material—are essentially the same at all levels of therapeutic work.

There is one additional point I should like to make. Many educators—notably A. N. Whitehead (3) have felt that one of the major university functions is the stimulation of imagination. "The justification for a university is that it preserves the connection between knowledge and the zest of life, by uniting the young and old in the imaginative consideration of learning. The atmosphere of excitement, arising from imaginative considerations, transforms knowledge. A fact is no longer a bare fact—it is invested with many of its possibilities. It is no longer a burden on the memory; it is energizing as the poet of our dreams and as the architect of our purposes." In conducting the group sessions we try to break down the barriers between the students and the teacher. The group members—with the exception of the resident who has responsibility for the patient—are free from responsibility and have no responsibility for immediate action—one of the requirements Whitehead believes essential for the discipline of imagination.

I hope you will pardon my spending so much time on the details of the

method. The differences between this type of teaching and the conventional type are often difficult to see—yet the difference lies precisely in the details—and I believe that these differences are crucial.

The teaching method itself is primarily that of discussion, in which the instructor participates as little as possible. His job is that of a catalyst—occasionally that of a resource person. He sees to it that pertinent topics get into the discussion and that they are kept alive and left open-ended. Occasionally he shifts to a more didactic approach—but usually leaves the collection and dispensing of information to a resident or to a student member of the group.

In this type of teaching it is essential to centre the discussion on raw data—in this case it is the verbatim material of the interview. This material is obtained as accurately as possible—in our present set-up we are able to record the interview on tape and then listen to it directly or to a typed transcript of it. Occasionally the interview is observed through a one-way mirror and notes are added to the typed transcript in order to include more non-verbal material. In other words we try as much as we can to get at the precise events as they occurred in the therapeutic session. We do not want a paraphrase of the material, nor a summary of the material nor the doctor's impression of the material. It is the exact interchanges as they occurred that we wish to use for teaching purposes. I may add that an interesting chapter might be written on the resident's resistances which block the collection of this kind of information. Many complain that they cannot write and listen to a patient at the same time. Others can write down what the patient says—but always forget to write down what they say. Some forget to bring along a pen—and in the hands of other residents the pen is always running out of ink at the crucial moment. An occasional resident complains that the patient will not allow him to write. In our present set-up where it is possible to record an interview through a microphone in the ceiling—the resident forgets to throw the switch or he talks too low or mislays the tape. Many are the ways of resistance. However, if the instructor persists and is tolerant about the ways of the flesh—and especially if we are able to show the resident that even though the collection of verbatim material is not necessary for therapy to occur it is crucial for teaching and for scientific study—the student usually succeeds in bringing material to the group which can be utilized. This is the material from which the problems evolve, and it is examined by the group repeatedly for purposes of sensitivity training and to develop skills in observation. Focusing the group's attention and interest in the same area is done in order to force intensive data collection. In other words the class performance becomes task-oriented behaviour.

We have used these teaching methods in teaching all kinds of psychotherapy. In our work with residents and more advanced physicians our major effort has been in teaching the type of insight therapy I have briefly outlined above. The basic formulation of the method is to allow the resident to have an experience in working with a patient, scrutinize the experience, draw inferences from it and then generalize about it. We have made considerable use of other teaching methods. The demonstration of interviews, lectures and abstract discussions of therapy have their place. I personally believe that these are secondary methods, and are to be used only when the type of teaching described above cannot be undertaken. Many teachers have reported that they have found the series of sound films that Dr. Powdermaker and I have made useful to them. We use them after our residents have had the kind of training described above. Students tend to copy their teachers and to try to adopt the techniques used by them. It is very difficult to avoid this in one's students. We would much prefer to

have the student grasp the principles of the method of therapy, try it on his own—and adapt the principles to his own personal techniques of communicating with people. I also go along with John Dewey's idea that *demonstration* generally emphasizes the possession of knowledge and blocks the penetrating inquiry necessary for the discovery of new facts.

We have found it useful to define psychotherapy as a treatment in which the therapeutic agent is psychological (4). It is a treatment in which psychological means are used to remove symptoms, to resolve conflicts, to improve the patient's internal and external adjustment, to allow the patient to have the maximum use of his resources—and to prevent further illness. These are the ultimate goals. We believe that in psychotherapy the efficacy of the psychological agent depends in part on the meaning it has to the patient. Adolf Meyer once said that psychotherapy begins as soon as we see the patient. There is no doubt that psychological processes are involved in every human interaction between people and between the doctor and the patient—no matter what the purpose is. We would however prefer to limit the use of the word psychotherapy to planned therapy—and not use it for beneficial changes which occur in the patient unbeknown to the doctor and often in spite of the doctor. In each type of therapy there are immediate objectives and procedures useful in achieving these objectives. We have described the immediate goals, the procedures, and some therapeutic results elsewhere. Our major effort in teaching psychotherapy is in teaching the type of insight therapy previously described.

I should now like to report on another experience in teaching psychotherapy. Since I came to the University of Maryland seven years ago our department has had the responsibility of teaching psychiatry to undergraduate medical students throughout all four years. We felt that we would like to teach the kind of psychiatry which would be useful to doctors in working with their patients—which would help students to acquire and develop a therapeutic attitude toward patients, which would help them get patients to communicate freely. We wanted to teach the skills necessary for the development and utilization of a therapeutic doctor-patient relationship and the skills necessary ultimately to do psychotherapy.

In contrast to the psychiatric resident the medical student presents different problems. The resident (5) is usually "sold on" psychiatry—often has an uncritical acceptance. He usually considers psychoanalysis as the model therapy and is impatient of other kinds of therapy, which are considered superficial. The psychiatric resident is usually uninterested in the physiological and behavioural areas, and at present most residents, I believe, are not research-oriented.

Most students come to medical school equipped with information in limited areas, but usually unaware of how many kinds of knowledge and what diverse skills in thinking and performance are essential to the intelligent practice of medicine. The student usually does not associate the psychological, the social, and the interpersonal experiences with illness, treatment, or prevention. Nor does he assume that these relationships are matters for objective study. The same student who accepts his need to learn physical diagnosis, surgery, epidemiology, does not readily grasp that his professional competence hinges on his learning the subtle skills required in interpersonal malfunctioning. His interest and understanding may be actively blocked in these areas by prejudices ranging from vestiges of magical thinking and outmoded concepts of what makes people behave to a complacent and conservative so-called commonsense approach. Even the sophisticated student usually has need for an ongoing orientation as

well as for training in applying these ideas to the problems of patients.

One of our goals in teaching medical students—and we begin to work towards this goal as soon as we get sight of the students in the first year—is to get them to explore and to try to understand what are the building blocks they are to use in their work with patients. It seems essential to break down the barrier so that the student can move back and forth with facility from the physical and biochemical to the psychological and interpersonal levels. This flexibility in what we call horizontal locomotion is necessary for the student to be able to set up for a given patient at a given point in time a hierarchy of factors, and an awareness of their relationship. In considering any one area of subject matter another one of our goals is flexibility in vertical locomotion—so that the student can move up and down from fact to theory, to pre-supposition, to guess critically but with understanding and humility. We should like the student not only to recognize the role of the many factors in the aetiology and maintenance of disease and maladjustment but to be aware of the limitations imposed upon the doctors' actions by the present state of knowledge. All this is to occur in an atmosphere of critical acceptance.

To achieve the above is indeed a tall order, and yet these are by no means the only goals. Many other parameters which have not yet received adequate recognition in medical education enter the picture. These deal with value and status. Our concepts of adequate medical care, the economics of medicine, our attitudes toward patients and colleagues are deeply imbedded in our value systems. These concern personal as well as social values. Without such considerations it is, I believe, impossible to be intelligent about the moral factors in illness, and treatment, and about the role of the physician as an agent of society. One other area I shall merely mention—that is the attitude toward information and the process of obtaining and validating facts. This does not imply a formal course in epistemology—but it does imply the recognition of what we mean by a scientific frame of reference. The student must understand not only what we know but how knowledge in medicine and psychiatry comes about, and what are the ground rules of man's attempts at fitting the contours of nature. He must avoid the inept attempts at measuring words and feelings in a test tube and at the same time check his imagination by paying attention to methods of inference testing (4). He needs to understand that when we cannot measure we can still use definitions (6) and still can describe. It is the appropriate use of the best available methods for the study of all problems—not the slavish acceptance of a specific method or the complete surrender to doctrinaire theory—which is the essence of scientific inquiry.

One might well ask what has all this to do with the teaching of psychotherapy. The orientation and attitudes described above are necessary in the teaching of all subjects in medical school and elsewhere. Yet, few departments in the medical school are at present concerned with teaching attitudes and orientations. To be sure, students are identifying all the time with their teachers, some of whom through example are living models of the complete physician. This is necessary, yet not sufficient. We want the use of selective identification to occur. We want the student to identify with the good in us and permit the evil in us to be interred with our bones. To achieve this requires more than exposing the student to the ideal teacher—he must be aware of the learning process, discriminating what he learns from the person who does the teaching.

In attempting to answer these questions within the confines of the competitive pressures of the medical school during the past ten years we have groped for a methodology. My own personal experience in doing many kinds

of teaching has made me feel that the conventional didactic teaching, useful as it is, does not fit the bill. Whether our lectures deal with neuropathology, or psychodynamics, or psychoanalytic theory—or feedback systems—in my opinion they leave much to be desired as a means toward our goals in medicine. The presentation or demonstration of a patient even to the first year students, the use of an interdisciplinary team of teachers, or even the didactic teaching of correlations between pathology and personality development and emotional precipitants or interpersonal events, represent a broader approach, but still are inadequate.

I should like to describe the first year course in psychiatry. Our first year class consists of about 100 students who in 1956–57 met for $1\frac{3}{4}$ hours each week during both semesters, about 32 sessions. In addition to the instructor, an internist, Dr. E. T. Lisansky, and a philosopher, Dr. John Reid, are usually present. At every meeting of the class a patient was presented from the medical or surgical wards. There were three requirements for the selection of the patient—that he have a common medical or surgical illness, that he be a patient in treatment with one of our fourth year students in psychiatry, and that he be not seen in advance by the instructor.

The course in 1952 began with a statement by the instructor—"According to the catalogue, this is a course in psychiatry. I wonder what you mean by psychiatry?" One student attempted to define psychiatry as a study of abnormal mental processes. Another student stated that psychiatry deals with mind. Another student brought up the idea that it deals with psychic. After further discussion another student mentioned that psychiatry is a study of sick behaviour. I encouraged further discussion by pointing out that these ideas are not very clear—maybe we can work out a more precise formulation so that we can understand what we are talking about. The discussion shifted to the mind-body problem. The gist of the discussion was that you really can't observe the mind. All we can talk about is behaviour—the behaviour of sick and well people. But behaviour is used very broadly to include not only the activity we see in a patient, but also the verbal behaviour—which tells us about the patient's feelings, ideas, attitudes, and problems. This was elaborated by several students, who pointed out the advantages and disadvantages of such a concept of behaviour. One student came back to the distinction between sick behaviour and normal behaviour. Another brought up what do you mean by normal behaviour? I referred to a chapter in Maurice Levine's book, in which he summarizes several definitions of normality (7). They discussed normal distribution curves—the use of normal as ideal and the problems inherent in this concept.

I asked the class if they would like to see a patient. There was an immediate flare-up of interest—I then asked what they would want to know about the patient. One student mentioned he would like to know about his dreams; another, about his problems; and a third, about his history. I asked, what would you want to know about his history? One student mentioned that he would like to know how he got sick; another, what kind of treatment he was getting. I asked if they would want to see the patient without hearing his history first—many students said no. I told them that I did not know the patient's history either. Would they want to see how I go about working with a patient whom I had never seen before? They hesitated—and agreed they would do so this time. I promised that we would hear the history after we had seen the patient. At this point, through a misunderstanding, the patient, a young male negro, was brought in before the class. I turned to the patient and said, "Would

you please mind waiting in the anteroom for a few minutes?" The patient said, "Alright", and walked out with the nurse. I then turned to the class and asked why I had asked the patient to leave the classroom. The discussion centred about what to say in front of patients. These first-year students discussed ward rounds—what they had heard about patients' reactions to disturbing statements coming from the visiting doctor, the resident, and the nurse. The discussion led to the problems of the confidential nature of the patient's communication. I added these were all problems of importance in medicine and were factors in the doctor-patient relationship.

I then asked if they would like to see the patient and asked them what they would want me to do. One student suggested hearing about his feelings, another about the history of his sickness. Another student suggested we find out about his dreams—why, I asked—because, he said, this is a course in psychiatry. Still another suggested finding out his problem and how he came to the hospital. Several students thought this was a good idea—and I agreed to do so. We discussed the problem of setting up goals in the interviewing situation, and I merely mentioned the ultimate goal for our work with patients in contrast to the immediate goal for the present interview. I asked the students how I should go about finding the patient's problems. They suggested I ask questions—what kind of questions?—or should I prefer to avoid asking questions? This gave us a chance to discuss leading questions, double questions—long, involved questions—specific or general questions. I then had the patient come in—introduced myself and the class—sat down before the class and had a ten-minute interview, focusing on the patient's description of his symptoms and the situation in which they occurred. After a few minutes I asked the patient if he would mind questions from the students. The patient said he would not—and the students asked a few questions—I would modify the students' questions, making them simpler—avoiding leading questions—and double questions. After the patient had left the lecture hall, I asked, what did they think? The first request was for the patient's history—I replied, yes—but first tell me what you noticed during the interview. One student said that I didn't do much talking—or didn't ask many questions; another student noticed that I did not achieve my goal even though I plugged at it. Another student noticed how the patient hesitated and looked down at the floor when I picked up the word "upset" that he had used. I asked, I wonder why he did that?—the students could not explain why he hesitated. Another student pointed out that he had noticed the patient also hesitated when he talked about his problems with his boss. Why? No one knew. We decided we could watch this phenomenon in other patients interviewed in the same setting. A student remarked that he noticed I called this negro patient Mr. Smith—and not John—why didn't I call a negro patient from North Carolina by his first name?—it was common practice down there. Several students brought up the problem of a negro patient in Baltimore. Should we call adult patients by their first names—even though they are negro and are treated on the wards? I told the story of a well-known Boston physician who could not understand why he always called ward patients by their first names—but for some strange reason, never did this with private patients. Several members had definite opinions why some doctors behave in this strange way. The discussion was brought back to a description of what happened during the interview—eventually the students began to add up what they had learned into building up a tentative diagnosis. They agreed this patient probably had arthritis. The diagnosis should also include boss trouble, hesitation in talking about "upset". He probably was avoiding talking

about his difficulties, he probably did not get a balanced diet in North Carolina, and maybe felt insecure among the white doctors in a large hospital.

The students then asked again to hear the history—and we asked the fourth year student who had been present throughout to tell the class what he had learned in working with the patient. The student—much to my surprise—turned to the class and said, “What do you want to know about the history?” A class member asked for past history. The fourth year student asked, “Why do you want to know that?” Many questions about the history were asked, and the fourth year student answered as best he could, only after the first year students had told him why they wanted to know a detail. After about fifteen minutes of this, the fourth year student presented the history, findings, diagnosis, and treatment. The fourth year student was asked to tell us next week what had happened to the patient. The class was insistent that the fourth year student try to get answers to their questions.

One of the many comments made by the first year students seems especially pertinent. One student said, “I can’t understand why you have us see a patient with arthritis—this is supposed to be a course in psychiatry.” I turned this question to the class. Several pointed out that maybe the patient with arthritis had some personal or social problem that was important in his illness. I merely added—“that’s one of the theme songs of the course”.

Discussion with maximum student participation formed the backbone of the method of teaching. Each session was begun by asking the students what they would like to talk about. The students were encouraged to bring up any topic they had in mind. This usually turned out to be a topic related to the interview. When a topic of interest was brought up, we attempted to have the class amplify the discussion and elaborate on the topic as much as possible. By “interest” is meant current interest on the part of the class, as gauged from frequent scanning of the group and through awareness of the behaviour of the students. At the same time we tend not to lose sight of the goals for the course, and to be aware of the extent to which the discussion was pertinent to these goals in topic or in approach. Through variations in behaviour, conveying or withholding interest in a topic, a technique derived from the doctor’s role in psychotherapy, the discussion was steered so as to keep it as close as possible to the motivating interest of the students and yet pertinent to the goals for the course. If the discussion took the form of a question, the question was usually not answered but turned back to the student or to the class—“What do you think about this? Perhaps someone would care to answer.” At times the same point was brought up during a session by different students. This was encouraged. When the opportunity to focus the discussion presented itself, we preferred to focus it on the problems of the patient, or the problems of the physician. In order to force intensive data collection and for purposes of sensitivity training, the attention and interest of the group was focused again and again in the same area. On many occasions the discussion was prolonged and the clarification of a point was delayed in order to stimulate the students to clarify the point themselves by more active intergroup discussion. Whenever possible, the observations were formulated as problems to be solved by the class. In other words, the class performance became a task-oriented behaviour.

The choice of this method was due to the special orientation of the instructor. It is our belief that the most meaningful teaching occurs when the student is involved in an experience of interest to him. We have felt that in teaching attitudes or a point of view, it is much more effective to have the student have an experience, scrutinize the experience, draw inferences from the

experience, and then, if possible, generalize about it. It would have been simple enough to describe the scientific attitude of the doctor, and we should have expected to have the students verbalize these attitudes. This was precisely what we wished to avoid. For this reason the emphasis was on observation and on participation in the brief interview of a patient. The students were encouraged to describe what they saw and heard. This occasion was used every week to stimulate their capacity for observation of such subtle matters as the patient's verbal and non-verbal behaviour, the gross and subtle interactions between the patient and the doctor, the vocal inflection of the patient and the sudden shifts of the topic in reaction to a remark of the doctor. After the class had described the observations, the instructor focused the discussion so as to force the students to make sense out of what they had observed. Often the students were able to see the appropriate inferences and even arrive at a theory. Topics were generally left open and alive. We avoided tying up or clinching a subject in order to keep it alive for subsequent discussion. Relatively rarely and for short periods of time was a more didactic approach used. Required reading and authoritative formulation were minimized, especially in the early weeks of the course. This was done to emphasize the importance of direct observation of the patients seen by the class. Later in the course a list of suggested reading was made available, with emphasis on reading from scientific literature—not from textbooks.

The major topics throughout the course were the doctor-patient relationship, methods of interviewing, and information on the psychological, interpersonal, and social factors in disease. Considerable time was spent on the attitudes, ethics, and values of the doctor, and on scientific methodology. The discussion was focused on the factors that promote or block effective therapy, on psychological mechanisms and defences. Again when the students attempted to push the discussion towards a formal presentation of psychodynamics, or of psychoanalytic theory, the discussion was directed into a description of the behaviour itself for which the theory attempted to account, or upon the psychological mechanisms as the basic theoretical constructs. Other content areas included personality development and psychoanalytic theory.

During the current course twenty patients were seen by the class essentially in the same way as described above. An effort was made to relate one case with another and to let the class point out the similarities and differences. Since the patients were seen in the same situation and since the behaviour of the instructor during these brief interviews was essentially the same, the student could get an impression of the range of activity seen in 20-25 patients. The students were encouraged to compare their observations based on their common classroom experience, and to draw appropriate inferences, differentiating between observation, inference, and theory. The function of the instructor was merely that of a catalyst—always making sure that the topic was covered as extensively as possible.

I fear that I have not done justice to my topic—I have left much unsaid and many questions with you. I do hope that they may come up in the discussion. I hope that I have conveyed the methods we have used in our teaching efforts and some of our thinking. We hope that the use of these methods may be of some help to physicians and to us psychiatrists in making us better therapists, sounder scientists, and more sensitive human beings.

REFERENCES

1. FINESINGER, JACOB E., "Psychiatric Interviewing. I. Some Principles and Procedures in Insight Therapy", *Amer. J. Psychiat.*, **105**, No. 3. September, 1948.

2. FINESINGER, JACOB E. and POWDERMAKER, FLORENCE, "Psychotherapeutic Interviewing Series" (films), Veterans Administration, 1949.
Part I: Introduction.
Part II: A Method of Procedure.
Part III : An Approach to Understanding Dynamics.
Part IV: Non-Verbal Communication.
3. WHITEHEAD, A. N., *The Aims of Education*, 1949. New York: The New American Library. p. 166.
4. REID, JOHN R., and FINESINGER, JACOB E., "Inference Testing in Psychotherapy", *Amer. J. Psychiat.*, **107**, No. 12. June, 1951.
5. FINESINGER, JACOB E., Discussion of paper "On Discovery and Experiment in Psychiatry", by Henry W. Brosin, *Amer. J. Psychiat.*, **3**, No. 8, February, 1955.
6. REID, JOHN R., and FINESINGER, JACOB E., "The Role of Definitions in Psychiatry", *Amer. J. Psychiat.*, **109**, No. 6, December, 1952.
7. LEVINE, MAURICE, *Psychotherapy in Medical Practice*, 1942. New York: The Macmillan Company, 1942, p. 320.

DISCUSSION

By Dr. Denis Leigh

In 1948 I spent twelve months at the Massachusetts General Hospital working in the department of psychiatry under Dr. Stanley Cobb. Dr. Jacob Finesinger was second-in-command and had been devoting himself to the technical aspects of psychotherapy, analysing the verbal, motor and expressional interplay of patient and doctor. One of the results of this was his well-known paper on "Some Principles and Procedures in Insight Therapy" which describes a method of psychotherapy since known as the Finesinger technique. It is therefore a particular pleasure today to be able to take part in this discussion of the excellent account he has given us of his method.

My own experiences are based on my work in charge of a Psychotherapeutic Unit at the Maudsley Hospital which I set up when I returned to England in 1949. In many respects this is modelled on the Harvard Unit, though there are, of course, differences. In the eight years since then, about sixty registrars have passed through this unit, each having a minimum of six months' psychotherapeutic training and a variety of foreign visitors have come to work with us. Undoubtedly the Finesinger method is the keystone in teaching and I regard it as the most practical method of teaching psychotherapy to beginners.

For the postgraduate there are several methods whereby he can obtain a knowledge of psychotherapeutic techniques. First he can pick it up, by practice, by trial and error, by reading, and by possessing a strong natural bent for psychotherapy. This method requires enormous determination and persistence and from my experience with such of my registrars who have been self-taught before coming to the Maudsley, it is only the exceptional person who can learn psychotherapeutic techniques in this way.

Secondly there is the personal or training analysis. Around London and one or two larger cities this may be a practical proposition, but over the great area of this country the facilities just do not exist. The difficulties are undoubtedly serious—the cost, the time, and the personal upsets which may occur have to be carefully considered in the light of the future plans of the analysand. But several mental hospitals around London have had numbers of their staff so trained, and the analytically orientated, to say the least, are now much more frequently encountered, and play a most helpful part in the work of the mental hospital.

Thirdly there is the type of training which is carried out at the Maudsley, and at one or two other psychiatric centres. The central thesis of this training is that psychotherapeutic techniques, both individual and group, are repro-

ducible, teachable and subject to intellectual analysis. It is with this third type of training, carried out, as I believe, within the framework of a busy and active psychiatric unit that I am concerned. All my subsequent remarks will refer only to individual psychotherapy—other physicians, notably Dr. Foulkes and Dr. Kraupl Taylor, are responsible for the teaching of group psychotherapeutic techniques: all their groups are formed from our out-patients.

It is surprising how, in spite of all the wealth of literature on psychotherapy, it has only been comparatively recently that attention has been devoted to the technique. An early book by Edward Glover *The Technique of Psycho-Analysis* was most revealing, if only in showing the wide diversity of techniques used in the psychoanalytic situation. The equivalent of an "Operative Surgery" simply does not exist—the nearest I know is the book by Wolberg on Psychotherapy. The dead hand of Freud perhaps still rests heavily on the subject—only by personal experiences can we master the techniques of psychotherapy. However attempts were begun in 1947 by Finesinger to describe some of the technical aspects of psychotherapy, and in a series of films, and a solitary paper he described a method of psychotherapy which experience has shown is particularly fruitful for teaching. This is his so-called Insight Therapy.

The method forms an admirable introduction to technique, and a basic skeleton on which to hang the flesh and blood of experience, but it has its drawbacks. Firstly, it is not a method which often produces insight; in fact a gain of insight, either intellectual or emotional, I have found very rarely occurs. Secondly, it is difficult to carry on for more than 20 interviews using a strict Finesinger technique, both for the doctor and the patient. Symptoms as a goal are most often very unrewarding, as the patient, by the time she comes to a psychiatrist, is stuck in a groove so stereotyped that each day's recital of symptoms is almost identical to the previous day's. Thirdly, the attempt to make interviews non-personal, as it were, is doomed to failure from the start, and transference material, as we all well know, can be seen from the first interview. To keep a young psychiatrist pinned down to one goal for week after week, as Finesinger did to me, is sheer cruelty and unrewarding in proportion to the efforts involved. But my first patient—the wife of a drunken Bostonian Irish slaughterman, with phobic symptoms—still sticks vividly in my memory, perhaps owing to the quite unsuccessful nature of the therapy and our mutual inability to proceed beyond "tell me about your symptoms".

Its great virtues are twofold. First in the setting of goals, so that from interview to interview the procedure can be planned, and therapy does not aimlessly drift along in an atmosphere of pious hope that something will come out. And second, in the use of recording. The insistence on some form of verbatim recording was comparatively new in 1948, and met with much opposition. Nowadays it is so accepted that a recording machine is almost the trade mark of a psychiatrist—at least in some circles. I have insisted on verbatim recording and tried most types of method, but for practical purposes I still regard the handwritten record as the most useful method. Of its drawbacks I am well aware, but for teaching purposes the written verbatim interview is ideal. This is not the place to go into the interesting technical details, nor my experiences with this and different methods—but I can only say that I do not think psychotherapy is teachable without some form of verbatim interview record.

The Finesinger method may be very suitable for training with in-patients, but it is not so useful with out-patients, at least when weekly or at most bi-weekly therapy is all that is indicated. On my own unit there is a marked

cleavage between the techniques used for out- and in-patients. The out-patient attends once a week by appointment for a 50-minute interview, and we aim at an average of 8-10 attendances. To carry out a useful psychotherapy under such conditions involves far more skill than is necessary with the relative inactivity of the Finesinger technique. A great deal more active intervention by the teacher is necessary in order to help the trainee. First he must make a psychodynamic formulation after the original diagnostic interview, and be able to present this in terms which are comprehensible to the trainee at the particular time—this is one of the tests of teaching and is always of interest. And secondly he must teach a different technique, depending primarily on the recognition of defence and patient-doctor feelings. I have always found the book by Alexander and French on Psycho-analytic Therapy excellent, and a paper by Coleman, published in the *Bulletin of the Menninger Clinic* is a very useful précis of the methods best fitted for this type of out-patient psychotherapy. Interviews are, of course, recorded, for without this teaching would be impossible, but recording in these circumstances is much more difficult than in the relatively peaceful Finesinger technique, for the doctor may be much more active. One of the most difficult things in writing down an interview verbatim is to record one's own sayings exactly.

Clearly the recognition of the patient's main defences is not easy for the tyro, nor is it easy for him to assess the patient's feelings towards him. But the main technical problem is that of when the doctor should intervene, and what form his interpretation should take. It is here that a knowledge of psychopathological theory is most useful—and lacking this to a large extent, the trainee must obtain more help from the teacher. Here is the place and the opportunity for a theoretical discussion of mechanisms. The trainee is given the problem of presenting to the group, let us say, the mechanisms of conversion, or of symbolization, he is given the references to look up, and all the group participate in a wide discussion of the particular point. Incidentally, this is a matter of some importance to our discussion today, for such a kind of training depends on easily obtainable sources. There must be a good library on the spot. At the Maudsley we are spoilt for choice—but it is not difficult to build up a selection of key papers or of books. I have used reprints considerably in teaching, when unobtainable I have had original papers Roneoed—a great help.

The selection of patients for this type of out-patient psychotherapy is another important point. Patients with sexual perversions I have found to be excellent subjects for teaching. The kind of sexual pervert I see in hospital practice has come largely to unburden himself and to be tided over some particular crisis. Eight to ten interviews along the lines I have mentioned, whilst of course producing no fundamental change, often help these unfortunate people, and at the same time, provide a classical illustration of some points of psychopathological theory. Again certain psychosomatic problems are very suitable, migraine, asthma, and some skin disorders, although again I have found the latter group to be notably unresponsive to treatment as regards the skin condition. Phobics and obsessionals are to be avoided, as are conversion hysterics—any patient where there is the likelihood of intense dependency reactions occurring should also be avoided. Frigidity and male equivalents, such as impotence and premature ejaculation have also been most unrewarding problems. I am mentioning these because I feel these experiences are very germane to the larger considerations before this meeting. One of the most important things I have learnt is that no more than a comparatively small

proportion of psychiatric patients can be treated psychotherapeutically, certainly within the framework of my own unit. I am not going to subscribe to the view that this is only because not enough time is available, or that some particular school of psychopathological theory provides results which the non-initiated cannot hope to produce. Whilst admitting that there is a great need for more psychotherapy over the country as a whole, I think it is important to stress this fact. The trainee must not be left with the impression, so anxiety-producing, that he is only doing something which is second best, he must be taught what psychotherapy cannot do—and, whilst in no way decrying the specialist psychotherapist, I do believe that the general psychiatrist is the person to do the vast mass of the psychotherapeutic work in our hospitals and out-patient departments.

These are the two basic types of psychotherapeutic training—in the techniques of brief out-patient therapy, and in the Finesinger technique. But during the six months the registrar must be given the opportunity of learning something of other methods. If possible, he carries one patient using hypnosis, and also uses intravenous methedrine for abreaction, and L.S.D. for somewhat special purposes—for instances we have treated severely phobic patients, or girls with anorexia nervosa who are comparatively inaccessible to ordinary interviews. Flexibility is the aim—to produce a psychiatrist who will have a broad, flexible and tolerant approach to psychotherapy, ready to adopt new methods if necessary, but not to be overwhelmed by them, and above all, to preserve his critical faculties.

THE NEUROTIC PROCESS AS THE FOCUS OF PHYSIOLOGICAL AND PSYCHOANALYTIC RESEARCH*

By

LAWRENCE S. KUBIE, M.D.

*Clinical Professor of Psychiatry
Yale University School of Medicine*

A SERIES of studies published since 1951 attempted to clarify what is meant by the neurotic in human nature. In returning to this problem today, I must beg your indulgence for repeating myself at some points. Apart from the kindness of your chairman in inviting me to discuss the Neurotic Process as my contribution to this symposium, my further justification is the opportunity which it gives me to add a few considerations which may round out the hypothesis and make it more useful, and also to indicate that the neurotic process is the meeting place for the organic and psychological approaches to problems of human behaviour (4, 5, 7, 10).

SOME REASONS FOR CONTROVERSY

Indeed it is a reflection on our maturity as scientists to find that a sharp cleavage still exists in a few medical centres between those psychiatrists who approach the problems of human behaviour with a psychophobia and those who approach the same problems with an organophobia. I use the word "phobia" advisedly to characterize the dominant feelings of the antagonists, because, like anyone who is dominated by fear, they become irrationally angry at those who force them to face what they fear. The "organicists" act as though they were terrified lest "It" might turn out to be psychogenic; while the psychologist (including the analyst) acts as though equally terrified lest "It" turns out to be organic . . . An agoraphobic and a claustrophobic have a hard time living together in holy wedlock. An organophobic analyst and a psychophobic neuro-physiologist have a hard time trying to make a joint attack on the problems of human behaviour. If this brief contribution undercuts the rationalizations with which these two phobia-ridden warriors try to justify their opposing positions, it will serve a useful purpose.

There are several human causes for this persisting obscurantism. Curiously enough some of them are biases which the two groups share unwittingly. First among these is the general fear of examining ourselves too closely. It is always surprising to find out how ambivalent we are about self-knowledge. Man has talked about it since Socrates; but he has done singularly little about it: almost nothing in education, and only a little more than that in psychiatry. This confronts us with a strange paradox. Every student of the behavioural sciences has at the root of his general interest in behaviour a more specific concern with human behaviour, and this whether he is studying human beings or lower animals. At the same time, underlying a general interest in other men's behaviour lies a more personal concern with his own. Thus the behavioural scientist's

* In Dr. Kubie's unavoidable absence the paper was read by Dr. E. B. Strauss.

study of rats or ants, or fish or ducks may be in part an unconscious and even obsessional defence against knowledge of Man. Or he may study other men as a defence against self-knowledge. We encounter this not only in those who are actively engaged in experimental work on animal behaviour, but also in the academic psychologist, in the clinical psychologist, in the psychiatrist; and let us not by any means forget the analyst who, like all other members of the well-known human race, can use his knowledge of others to screen his ignorance of himself.

CONSEQUENCES OF OBSCURANTISM

One of the most serious consequences of this is our failure to be as good naturalists as we should have been. We have by no means observed and recorded the phenomenology of human behaviour in the detail which is needed. We have behaved as though we were afraid to look. Studies of unconscious biases in other fields of science have revealed how this fear can distort the experimental work and blind the eyes of the scientist (9). Nowhere has it played a more subtly destructive role than in the behavioural sciences. This fact should make us humble in place of the partisan acrimony of the past and even of the present. In addition to dulling our vision, these biases have caused us to rush precipitately either into excessively complicated theoretical systems or into equally oversimplified experimental designs, without first pin-pointing what it is that we are trying to explain theoretically or experimentally.

As we examine these hasty and heated debates we find to our surprise that although psychoanalysts and neurophysiologists have the illusion that they are approaching the problem from opposite angles, they have been responsible for similar logical lapses.

1. Neither has described the critical step which initiates the process of falling ill.

2. Neither has distinguished between this initial step and the complex chain reactions which it launches. These omissions have confused our ideas on nosology, aetiology, and therapy.

3. Both sides have overlooked the full implication of the fact that each step in the development of the neurotic process becomes a source of secondary symptoms, which in turn have consequences which give rise to tertiary symptomatic defences which are the source of new consequences and on and on. This complex chain reaction should long since have led us to ask what transient cross-section of this continuously evolving process can be looked upon as a disease entity, i.e., as a specific type of psychoneurosis with a consistent aetiology and outcome.

We can speak of single or specific causes only in relation to the critical first step. After that the number of variables which combine to produce the chain of secondary and tertiary consequences increases by geometric ratios, because each new development has its own feedback effects. Let me reaffirm that this state of affairs mocks any attempt to search for single causes. Therefore our first responsibility is to isolate and describe the precise nature of this first step: something I will try to do below. If we fail to keep this distinction clear the resulting confusion is as great as it would be if the organic neurologist in his study of aetiology and of therapy were to make no distinction between the initial infection of the anterior horn cell by the virus of poliomyelitis, and the paralysed, atrophied, shortened and contracted muscles, the ankylosed joints, the spinal curvatures, and the difficulties in breathing and swallowing which follow. We call all of this "polio": but it is really a chain of events, each step

of which has new mechanisms, consequences, and therapeutic hazards and problems. In the same way, we speak of "the psychoneuroses" as though they were distinct clinical pictures; yet it is only the initiating step which is a nosological unit. Moreover we have not yet settled clearly the question of whether there is only *one* way in which the neurotic process can be initiated, or several.

Let me tell a story so familiar that there is not one of us who could not duplicate it from his family, his friends or his practice. Yet, like many everyday matters it contains the essence of every problem with which the unique phenomenology of the neurotic process confronts us.

My story concerns a child who is now 3, but begins when that child was 2. She was a precocious, enchanting, brilliantly loving and well-loved minx, whose development in every direction was free and spontaneous. Her speech and play, her affective relationships, all her instinctual processes developed with freedom and grace. At the age of two a small sister was born. For three weeks the older child clung with uneasy desperation to her mother's side, crying if she left, clutching eagerly when she returned, showing temporary uneasiness at night. This quickly subsided with the calm, persistent, steady presence of her mother, and presently she was as gay and free as she had been before. Towards the end of the subsequent year, the younger sister began to manifest the same enchanting spontaneity and the same precocity as the older sister. Then just as there was talk of a birthday party to celebrate the younger child's first birthday, the mother discovered that she had to go to the hospital for a minor operation. It was the hospital in which both babies had been born. Furthermore, the family had moved to a new home which happened to be near to this hospital, so that our young patient passed it daily on her way to the park. On this walk she would frequently encounter a great oil truck which challenged and held her attention as it stood at the corner, filling the storage tanks of the hospital. As the time approached for the mother to go to the hospital the older child again began to be disturbed. This time, however, something new entered the picture. This was not a purely affective process, like the first brief upset. This time there was also a disturbance in symbolic function. Every morning between three and four-thirty, the child would waken in fear. For the first few nights she said that a great big oil truck was coming into her room. Subsequently for the next weeks she only held long conversations for an hour or so until the dawn began to show, whereupon her anxiety would subside and she would fall back into a peaceful sleep. All day long she was gay and happy. But each night, as the new day brought with it the faint reminiscence of the day which had brought her sister from the hospital, the oil truck would become a symbol of terror of something barging into her room, into her domain.

Here we see the dissociations both in affective process and in the symbolic process which initiate the unique element in the neurotic process in human beings. We see both the neurotic potential which is universal, and the inception of the neurotic process. We do not see a fully developed neurotic state. What will happen subsequently will depend upon how such a minute episode is handled.

To clarify my point further let me give another example to indicate how different the ultimate clinical pictures can become in men whose neuroses had begun by identical steps. This is the great paradox whose importance for our explorations of aetiology and therapy has never been duly appreciated: namely that a process may start out by one or two uniform steps, yet end up years later in a thousand disguises.

Two men at fifty-five seem as unlike as any two men can be: yet their neuroses started out in life by essentially identical steps. Each began with pro-

found activity inhibitions which arose at the same age in reaction to basically identical circumstances. Later these diffused to become social and work inhibitions. In the one instance, because of a kindly patient encouraging grandmother, our patient could hide from the fact that he had never overcome the inhibitions which had had their origin at the age of three (as a response to the births first of a younger brother and then of a younger sister) and which had dogged his footsteps even since. Later in life he could hide himself in the office which his grandfather had staffed years before, and in which he could go through the motions of living the life of a man of affairs. The illness of the other man of fifty-five had also begun when he was about three, and was precipitated by almost identical events, with an activity block which, as it diffused into other spheres, made of school and college a series of endless defeats. This man, however, was hounded by angry and ambitious parents; and he reacted to this with despair and panic, then petty thievery, alcoholism, and ultimately a destroyed life. The first man lives in a state of mild, gentle, sweet and saddened misanthropy. The second thrashes around with explosive efforts to redeem himself, only to encounter overwhelming defeats, acute despair, escape into alcohol, reform, a melancholy drenched with tears and rum, and finally a super-imposed decompensation into an involutional depression with multiple somatic complaints. The cumulative, secondary and tertiary consequences of each step create two sharply contrasting lives and clinical pictures out of processes of illness which had started out by taking identical first steps.

I could also describe what is happening today in the life of a lad of thirteen who is in the midst of a struggle with the same problem as that which defeated this older pair, or another version of the same problem in a gifted musician of twenty-six. Over the years different clinical manifestations characterize each way-station. One is characterized by overt depression and anxiety, another by suspicion, another by compulsive obsessional defences, another by escape into alcohol or crime. Traditional psychiatry attaches a separate name to each of these way-stations in the evolution of neurotic illness. This hardly justifies our treating each as though it was a unique ailment with characteristic origins and aetiologies. We do not claim different aetiologies for pulmonary tuberculosis, tuberculosis of the bone, and tuberculosis of the meninges.

If this rather sweeping criticism is justified, then it becomes my responsibility to attempt to describe a better alternative. To this end, I will try to present a hypothesis, using a few diagrams which will indicate what I believe to be the essential disturbance in the human psychological apparatus. This will be a description and not an explanation, an effort to describe what deviations occur at the onset of the evolution of the neurotic process, but not *why* this happens. We cannot hope to explain the neurotic process until we can agree on the nature of the change which initiates it.

THE NEUROTIC POTENTIAL: THE NEUROTIC PROCESS: THE NEUROTIC STATE

I first described this in 1951 (4, 5); but the details have been subject to considerable development and modification in the intervening years. I can only hope that I will not fall into the very errors that I have been deploring.

1. There is a neurotic potential in every human being who is not feeble-minded. This potential is inherent in the symbolic process and its tripartite organization into conscious, preconscious and unconscious systems. At the same time it is this tripartite arrangement of the human psychological instrument which makes possible our highest creative achievements.

2. From this neurotic potential, the neurotic process evolves under the influence of many forces; some constant and universal, some variable. There are physiological constants and variables. There are external and internal experiences which are ubiquitous and yet variable; and others which are unique. Although the quality of the neurotic process varies, its existence is as universal as is the neurotic potential and as long-lived as life itself. Indeed, in this era of longevity we find the neurotic process as active in the 90's as in the first decades of life, and stamped with identical conflicts. Thus a woman of eighty-four still struggles with the neurotogenic conflicts with her siblings which had begun when she was four.

3. It is not an historical accident that our first intimation of this began with the psychoanalytic study of the clinical psychoneurosis; i.e., of the neurotic state. This was because although the fully-developed neurotic state is the *least* frequent manifestation of the neurotic process, it was the easiest to recognize by others and the most frankly acknowledged by patients. The neurotic state is painful to the patient himself, and because of this pain patients seek help. In contrast to this the subtler yet far more universal neurotic distortions of the personality hurt others more than they hurt the patient. Hence the neurotic personality rarely comes for treatment until a fully-formed clinical neurosis has crystallized, or else an affective or psychotic breakdown. At this point the pain which the previously unacknowledged neurotic process has caused to others boomerangs.

For example, an extraordinarily gifted, beautiful, highly endowed woman lawyer in her mid-forties came in tears. She said, "I have destroyed every important and valuable relationship I have ever had with a man. I have wrecked two marriages. I cannot pretend to myself that it is always the others who are out of step. It must be something in me that does it". And indeed this turned out to be true. It had been true in her childhood, in the way she had treated her young suitors in adolescence, in her relationships to fellow-students in law school and to colleagues in her law firm, even in her relationship to her clients, not to mention her more personal relationships. But it was not until her life faced disaster through the repeated rupturing of familial, professional and intimate relationships that she was forced to acknowledge those neurotic aspects of her personality, which to every close friend had been evident for twenty-five years. At this point the neurosis decompensated, bringing her to the edge of an early involutional depression. Before that no one would have been able to say that she had a single classical neurotic symptom. Yet the neurotic process had been at work throughout her life, compensated by her many gifts, so that until her breakdown became imminent only those who had been hurt by her had paid in their pain the price of her neurosis.

THE DEFINITION OF NEUROTIC

By now I have used the word "neurotic" repeatedly without defining it. In part this has been on purpose. I am no lover of definitions: to demand a definition at the beginning is like looking up the answer in the back of the arithmetic book before working out the problem. A definition which is imposed too early is a strait-jacket on free and exploratory thought, a convenient way of excluding everything which might make the problem inconveniently complicated, thus artificially over-simplifying it. Or if the premature definition is made all-inclusive, it becomes useless. We win our right to make a definition only as we approach the goal of our investigations. This is one of the many dangers of misused semantics. It gives even to so-called operational definitions

a deceptive allure, which frequently ends by begging all vital questions. Definitions have value only at the end of the chapter.

In this instance, however, I have already worked over this problem many times. Therefore I can attempt to define at least what is *not* neurotic. The word has nothing to do with any value judgments. The distinction between normal and neurotic does not hinge on whether something is good or bad, clean or dirty, selfish or generous, courageous or fearful, guilty or virtuous, constructive or destructive, implying activity or indolence, arrogance or penuriousness, ambition or apathy, ruthlessness or gentleness, conformity or rebellion, success or failure. It is not the value which attaches to the act nor its frequency or rarity which determines whether the act is healthy or neurotic. Whether any act is healthy or neurotic depends only upon the nature of the constellation of forces which determines it. If those forces, whether they be solely psychological or a combination of psychological and organic, are of such a nature that they *predetermine the automatic repetition of the act, irrespective of any considerations*, then that act is a neurotic act and the forces which determine it are neurotogenic. This is the essence of that which is psychopathological in human behaviour.*

What then determines this automatic repetitiveness? It is an interaction between two groups of variables, one organic and the other psychological.

On the psychological side it depends upon the occurrence of a process of dissociation which can operate in one of two areas. It can detach an affective state from its initial stimulus, so that the affective state continues ceaselessly to attach itself to other situations and objects. Or it can distort the relation between a symbol and what it originally represented, so that although we are conscious of the symbol the meaning has become inaccessible to our own introspection. Whenever this happens the pattern of thought, feeling, purpose or action becomes insatiable and repetitive. The oil-truck of the three-year old girl is a perfect example; since it stands for hospital, baby, mother's disappearing, the threat of a new intruder, etc. Once the meaning of the oil-truck was played out with the child, the nightly ritual of terror and chatter disappeared.

ORGANIC FACTORS IN THE PROCESS OF REPETITION

But what of the organic determinants of automatic repetitiveness? We know what organic variables may play a role in automatic repetitiveness. It

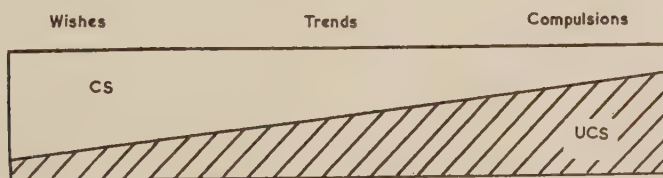


FIG. 1.—This diagram is a deliberate over-simplification, in that the area of pre-conscious symbolic processes (PCS) is omitted, so as to clarify the interplay between conscious (CS) and unconscious (UCS) functions.

Every human thought or feeling or act or pattern of living falls somewhere along such a continuous spectrum as this. The technical and quantitative problem is to determine where. It will be noted that the diagram indicates that there are no acts in which UCS processes play no role, and no acts which are devoid of CS determinants. If this is true, then in all probability the ends of the scale are theoretical abstractions.

* We exclude those stereotyped repetitions which are due exclusively (or predominantly?) to organic changes; because they constitute a separate problem.

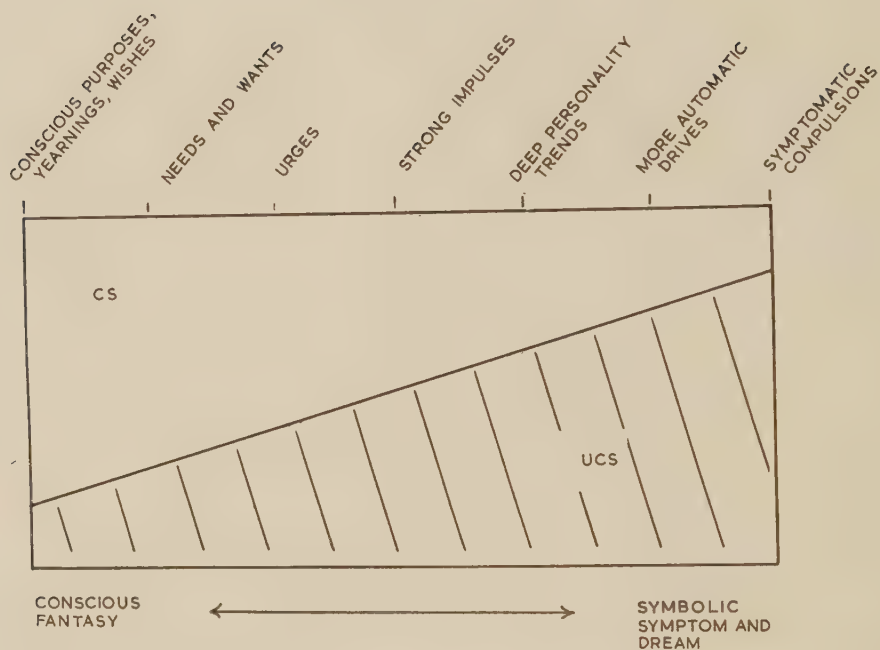


FIG. 2.—This diagram is still an over-simplification, in so far as it omits pre-conscious processes; but extends the implication of the balance between CS and UCS processes from the area of symptom formation to more general drives and personality traits.

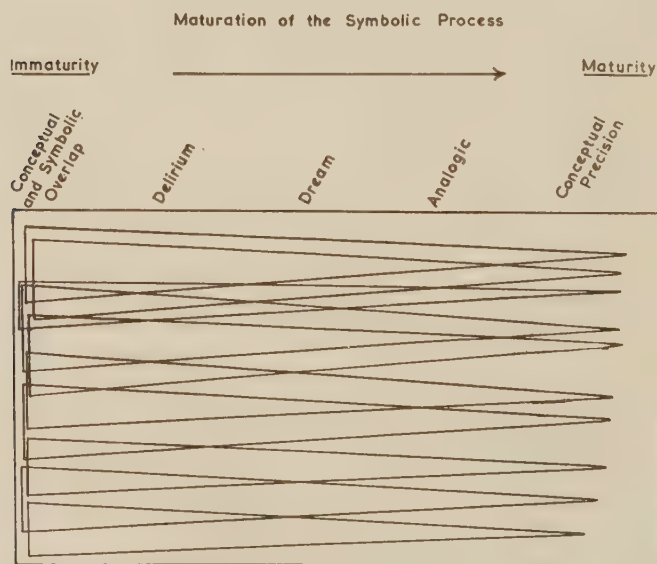


FIG. 3.

Each symbolic sound or gesture can represent more than one object or event.

Units of experience have acquired more than one symbolic representation.

An approximation to a more precise and restricted relationship between units of experience and their coded signals.

has long been known that the stimulation of certain areas of the brain can produce repetitiveness. This was demonstrated many years ago by Brickner (1) and more recently by Penfield (16). It was my privilege to give an example of this in a paper in which I used observations which I had made at the operating table in the Montreal Institute through the hospitality of Dr. Penfield (6). Indeed, our modern knowledge of the nervous system indicates that there is a tendency in the human nervous system to continue the same pattern of action until some new force enters the field of forces to alter its speed or rhythm, to divert it into new patterns, or to extinguish it. Our problem is to sort out the normal from the abnormal manifestations of this inherent tendency to repeat (cf. Max Levin (13)).

We also know that pharmacological agents can release manifestations of this repetitiveness. The alcoholic in the corner pub, who weeps into his mug of ale as he tells his story over and over like a broken phonograph record, provides a simple demonstration of this. Even in milder degrees, that type of repetitiveness is, of course, one of the striking patterns of lighter degrees of alcoholism. But the question still remains unanswered—because it has never been adequately explored—whether *all* individuals are equally prone to such repetitiveness when the concentration of alcohol in the blood stream attains some critical level for certain periods of time, or whether some nervous systems are more vulnerable to this type of pharmacogenic repetitiveness than others. Similarly we must determine whether those individuals who tend to manifest repetitiveness under alcohol are equally prone to automatic repetitiveness without alcohol. Would they, for instance, manifest this tendency in the stream of their free associations, if these were recorded for quantitative study in that controlled situation known as the analytic process? Or would they manifest the same repetitive tendency in the form and content of spontaneous dreams, or in the patterns of their work, play, loves and hates, or in their affective states in general? And contrariwise, do the so-called “tranquillizers” lessen the tendency to repetitive phenomena whether spontaneous or under alcohol? Here obviously is an area in which a combined investigation is urgently needed, which would employ the investigative techniques of neurophysiology and of psychoanalysis, and of clinical psychology. (This is a problem which, through the generous assistance of the Smith, Kline & French Foundation, I have begun to study during the past year.)

Again, do individual nervous systems vary in the ease with which they can set up reverberating circuits (a topic which I first had the pleasure of discussing in this hall in 1929 (3)). Is the tendency to repetitive phenomena something which is influenced by endocrine, metabolic or enzymatic differences, or by differences in the relationship between conduction-times, refractory periods and synaptic delays? Here, again, three-pronged research should be made, employing test instruments specially devised for this purpose by clinical psychologists, employing also the quantitative and qualitative study of recorded free associations in the standardized and relatively controlled situation of the analytic chamber (8, 12), and also using every possible neurophysiological and neurobiochemical technique. No one of these three disciplines working alone can find the answer, because any isolated attempt will leave two other orders of variables uncontrolled.

Indeed, it is for this kind of work that we need special institutes for Basic Research in Psychiatry (11). I have only contempt for the naïvety implicit in the recent rash of meaningless and premature investigations of whether you get 5 per cent. or 10 per cent. of improvements by one or another form of

therapy. What we need are basic investigations of the nature of the processes by which we fall ill and fall well.

THE CENTRAL EMOTIONAL POSITION

Another important variable in the neurotic process which demands joint psychoanalytic and physiological investigation has been vividly demonstrated in the observations of Spitz on the emotional responses of infants to separation from the mother during the first year of life (18, 19, 20). Spitz had demonstrated how, in early months, the infant's affective state, his nutrition, growth, and neurological maturation depend upon warmth, stillness, comfort, monotony, peace, closeness and especially on being surrounded by familiar objects, both animate and inanimate. These are epitomized first by the mother's face and the interrelated visual, oral and olfactory experiences of the mother's body and breast, by the slow play of familiar sensory stimulation from handling, cleansing, feeding, cooing, rocking, carrying, etc. Into this infantile constancy come the primary effects of inconstancy, i.e., separation, long-term or short-term, plus the impact of startle, neglect and starvation, cold and heat, sudden changes of light and dark and noise. All of these make a profound and lasting impression both on the emotional organization of the infant and on his physiological development.

From this and similar studies it is possible to make one certain deduction: namely, that early in life, sometimes within the earliest months and sometimes later, a central emotional position is frequently established. We do not yet know whether this *always* occurs or only in some infants, nor how early it can occur, nor how late, whether there is a variable intensity factor, or how the quality of the central affective position varies. Nor do we know whether this central affective position becomes structuralized. Whatever the ultimate answers to these questions, the clinical fact which is already evident is that once a central emotional position is established early in life, it becomes the affective position to which that individual will tend to return automatically for the rest of his days. This in turn may constitute either the major safeguard or the major vulnerability of his life. In fact the establishing of a central emotional position may turn out to be one of the earliest among the universals in the evolution of the human neurotic process, since it may start even in the pre-verbal and largely pre-symbolic days of infancy.*

Many other universal early experiences, acting at different stages in our development, contribute to the establishing of the central emotional position to which the personality returns automatically from any excursions to other emotional states. This central position may, at times, be pleasant and comfortable; a fact which is largely neglected as an object of psychopathological inquiry. Such an individual may spend his whole life unwittingly attempting to recreate, recapture and preserve this early euphoria, using many conscious, preconscious and also unconscious devices in his efforts. (Incidentally, this is a story which I have encountered repeatedly in a certain type of alcoholic.) Contrariwise, whenever the central emotional position is painful (e.g., a state of chronic masked fear, depression, rage, etc.) the individual may spend his whole life defending himself against it, again using conscious, preconscious and unconscious devices, whose aim it is to avoid this pain-filled central position.

This is a matter which is of more than academic interest. It raises several basic questions: are there inborn tendencies to one or another central emotional

* I wish specifically to point out that this has nothing to do with the science-fiction of Melanie Klein.

state? Or if and when they are acquired, to what extent and under what circumstances do these central affective positions become structuralized in the nervous system? To what extent are they alterable later in life by purely experiential devices (i.e., through psychotherapeutic techniques alone), or alternatively through organic therapies? What can be done early in life, whether by organic or psychotherapeutic methods, to free the developing personality from the inflexible domination of any central emotional position and from the equally inflexible influence of unconscious defences against painful affective tensions, or against anything which threatens an infantile euphoria?

I raise these questions without any suggestion that anyone knows the answers, whether he is an analyst or an organicist. Any analyst who has worked hard and long with such patients has had experiences so deeply disappointing that he comes away shaken by the conviction that the obstinate tenacity of these central emotional positions must indicate that they have become organized on a structural, enzymatic or biochemical level. Then, suddenly, perhaps the very next day, some other patient with what seemed to be an equally deeply rooted central affective position will undergo a process of remarkable affective liberation during psychotherapy. One patient recently said to me, "I feel truly reborn . . . As I look out on the world, everything I see has felt different to me ever since then" . . . He referred to a recent phase of analytical work. Such an experience proves nothing. It is only an honest report on a sequence of internal events. Whether the analytical work induced purely psychological or purely physiological changes or both, one can neither affirm nor deny . . . The same problem is raised by the sudden affective change which occurs "spontaneously" in any so-called Manic-Depressive as he moves from a depression to an elation or vice versa, without any recognizable change in his internal or external problems. These are further unsolved problems in which the physiological and the psychological scientist must become allies instead of enemies.

THE PROBLEM OF TRIGGER MECHANISMS

There is another aspect of the neurotic process which demands this combined attack. It is closely related to the problems of the central emotional position, but not identical with them. This concerns the many subtle and usually overlooked trigger mechanisms, which precipitate human beings out of one psychological state into another. These trigger mechanisms are among the most ubiquitous, the most unique and certainly the most neglected aspects of the human neurotic process. Psychiatrists have confined their attention to the one easily recognized example of trigger mechanisms, i.e., the *phobia*. Yet many other affective phenomena are triggered off in every-day life: such as laughter, tears, rage and anger. There can also be a triggered return to a central emotional position. Apart from affective processes, obsessional and compulsive furors can be released by triggers, furors of erotic or destructive impulses, furors of perverse activity, and furors of obsessional and compulsive symptoms which are defences against such drives. There can be the triggering off of sudden states of blocking and of dissociation. States of hypnosis can be set off by trigger mechanisms. Sleep itself and its disruption are responsive to triggers. Clearly triggers play a role not in affective states alone, but also in many complex psychological processes, both normal and pathological. About them we raise the familiar questions: are these activities inherent in the organization of the human central nervous system? Are there psychological, physiological or biochemical variations in the vulnerability of different human beings to trigger mechanisms? How do trigger mechanisms arise other than by the pedestrian

steps of laboratory conditioning? What is their relation to traumatic experiences, and to symbolic dissociation? Under what circumstances will sleep itself alter the vulnerability or response to trigger mechanisms? And how do trigger mechanisms vary in their responses to psychotherapeutic and physiological devices?

THE ROLE OF THE PRECONSCIOUS SYSTEM IN THE NEUROTIC PROCESS

Another oversight which has hampered our exploration of the neurotic process has been our failure to pay adequate attention to preconscious functions. Freud sensed their basic importance early; but because his attention was diverted into other directions, most of his later work and that of almost all other analysts emphasized almost exclusively the role of unconscious symbolic processes. The essential role in the neuroses was correctly assigned to unconscious processes (which is entirely valid); but many other normal aspects of human psychology as well were assigned to unconscious mechanisms (such as primary process thinking and instinctual derivatives (Id)). It is my suspicion that as a consequence of this, we have failed to recognize that by far the largest share of human perceiving, recording, thinking, recalling, problem-solving, planning and desiring, plus all of their affective concomitants and controlling influences, occur continuously and incessantly without the participation of conscious awareness. Yet they are not unconscious, in the technical sense. They are not behind an iron curtain of repressive forces. Of this incessant turbulent stream of preconscious mental activity our conscious symbolic thinking represents only a small sample. A basic gap exists both in analytic theory and in all experimental physiological investigations of thought processes from the failure of both disciplines to recognize the central role of preconscious functions in all human perceptual and conceptual processes, in identifications, in creative processes and in the psychotherapeutic process itself. Moreover it is the vulnerability of preconscious processes to distortion by unconscious processes which is the essence of the neurotic process: yet preconscious mentation has been almost totally neglected by analysts and neurophysiologists alike.

Ever since the work of Pözl in Vienna (17) and the recent investigations of Fisher (2), of Lindley (14), of Marsh and Worden (15) and others, it has become increasingly clear that preconscious processes play a role in all human psychology, which is as unique as is the symbolic process, the repressive mechanism and the unconscious process itself. In contrast to preconscious "processing" of thought, communication through symbols is a slow and pedestrian process. Furthermore our conscious symbolic functions do little more than sample this continuous and incessant preconscious stream, representing partial summations of everything that is going on preconsciously in the thinking and feeling apparatus of the human being. There is a continuous preconscious processing of experience through coded signals, which are not transmuted into conscious symbols for communication. This goes on during sleep as well as in the waking state and in every "intermediate" state between sleeping and waking, and always at a rate that is many times as rapid as is the speed with which either the conscious or unconscious symbol can be used. Evidence for this is based upon experiments with the tachistoscope, upon the data from hypernesia under hypnosis, on the data concerning the creation of works of art and the solving of complex mathematical and scientific problems in the single flashing image out of a dream, which condenses hours of work into a single hieroglyphic which passes in a flickering instant. It is this preconscious process

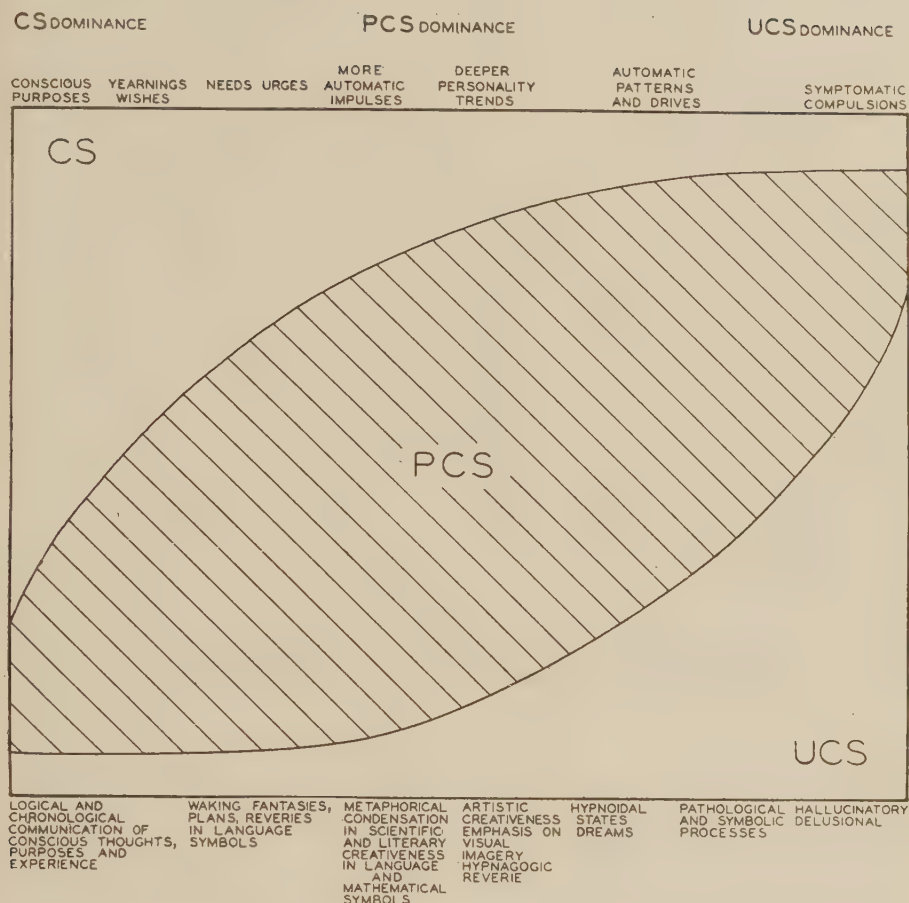


FIG. 4.—These relationships are represented crudely by this diagram. Figures 1 and 2 were purposefully simplified by omitting PCS symbolic functions. This represents hypothetical relationships in something approximating their full complexities. The shapes of these curves, their points of origins and insertion, the areas they subtend are all hypothetical. Even the implied assumption that their interrelationships can be expressed in quantitative terms is only a working hypothesis. Therefore the diagram is not to be taken literally. It is designed rather to illustrate complex interrelationships, as they may be assumed to occur in nature (i.e., in human nature), in which the concurrent action of preconscious processes frees our psychic apparatus, and more specifically our symbolic processes, from rigid anchorage. At the conscious end of the diagram this anchorage is to fixed and literal relationships to external realities. At the unconscious end of the diagram, there is if anything an even more rigid anchorage to unreality: because the unconscious symbolic relationships which dominate at this end of the spectrum are unmodifiable by experience as long as they remain unconscious. The flexible and creative contribution which is made to our psychic processes by the concurrent play of preconscious processes is illustrated in the middle band.

which is the one freely creative instrument of the human psyche. It can be imprisoned by processes emanating from both the conscious and unconscious ends of the spectrum. It can be anchored to pedestrian reality at the conscious end; and it can be distorted by the influence of unconscious processes at the neurotogenic end.

In the time at my disposal here it is impossible to discuss fully just how the preconscious process is separated out from conscious and unconscious

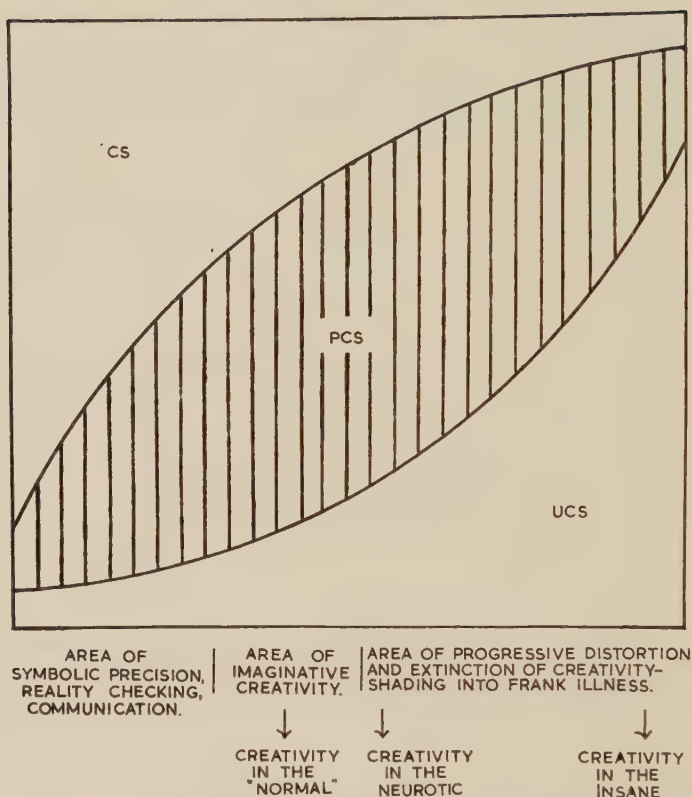


FIG. 5.—This figure extends further the implications of this diagrammatic representation of the interrelationships among the three systems of the symbolic process. It is an attempt to show at least one way in which the variations in their relative roles may explain the distortions of creativity under the impact of neurotogenic UCS processes.

functions; how it becomes the great economizer of the psychological apparatus in general and at the same time its freest instrument. It suffices to say that it is the great economizer, and our automatic creative implement. It is that aspect of human mental processes which makes the difference between an adding machine and an electronic computer, which can receive, register, process, and solve problems by means of coded signals and without the slower transmutation of the preconscious signal into conscious symbols. Only those coded signals which represent the final steps in problem-solving are transmuted into conscious symbolic devices (speech, gesture, etc.) for communication. And precisely here is where the critical dichotomy occurs. The links between some of these conscious symbols and the data they represent are known to us. These we call our *conscious processes*. In others, an obscure device called "repression" (which is not well understood although it has been studied and reproduced experimentally) severs or distorts the link between the symbol and its roots, so that what the symbol represents becomes inaccessible to self-inspection. Together the two symbolic representatives of the preconscious stream represent only a small and relatively slow sample of that stream, whose incessant turbulent flow is the unique source of creative thinking in human psychology.

Preconscious functions exert many influences on the psychological superstructure of the neurotic process. They play a role in directing the processes of

automatic imitation which determine the identifications of early childhood and which evolve later into those complex distortions of human relationships which, in analytic terminology, are variously called identification, introjection and incorporation (without clear-cut precision as to what extent these terms are over-lapping and to what extent they identify slight variants on the same central theme). In turn this plays a vital role in the shaping of personality.

The role of inherent differences among individuals in their capacity to function freely on a preconscious basis has not yet been ascertained—nor whether there are differences in the inherited preconscious equipment of writers, painters, musicians, athletes, or mathematicians, or in the link from preconscious percepts to automatic reactions. Yet all such differences, if they exist, must exercise a profound influence on the form, content and intensity of manifestations of the neurotic process, and on vulnerability to it.

This, then, is some of the equipment which the individual brings to his neurotic process. These are variables which no sane psychoanalyst or neurophysiologist can leave out of his thinking. Certainly Freud had such variables as these in mind, and referred to them many times without identifying them. Yet because analysts tend to work on the psychological super-structure with such absorption, we often forget the importance of these underlying variables. They are important not only because they help to determine the development of the neurotic process, but also because they vary enormously in their responsiveness to various types of psychotherapeutic or pharmacotherapeutic approaches. It is foolish, therefore, to think that everything which contributes to the neurotic process is going to respond identically to any one type of manœuvre.

It would be fallacious to take it for granted that because something has developed under the cumulative impact of experience it can be altered only through experiential devices, i.e., through psychotherapy. It would be equally fallacious to maintain that against the physiological variables the psychotherapeutic techniques must necessarily be important, or that where organic variables played a dominant aetiologic role only organic forces will be effective. Above all, it is fallacious to be unitarian in our attack on any of these problems, either from the point of view of aetiology or of therapy. Yet any effort to combine physiological and psychological methods into one co-ordinated attack involves many strategic difficulties. It is hard to avoid mutual interference between the two, hard also to avoid clouding our judgment as to which variable has been effective.

SUMMARY

There are many similar ways in which human behaviour and the behaviour of the lower animals can be disturbed. But there is an aspect of the neurotic disturbance of human behaviour which is uniquely human.

This is the dissociation and diffusion of the affective reaction from its original affective stimulus, and the dissociation which results from the repression of the link between experiences and the symbols which represent them, so as to render them inaccessible to conscious self-inspection. These constitute unique mechanisms of the neurotic process as it manifests itself in the human being. There is no evidence that anything comparable to these occurs in animals which are disturbed by laboratory procedures to produce what by analogy are called "experimental neuroses". In these experimentally induced disturbances there is only the secondary affective disturbance which in varying degrees is an invariable concomitant of the neurotic process in the human being, but never its essence.

It is of basic importance to specify at what point in the development of the personality, and in the development of the individual's capacity to deal with reality, and in the course of what struggles with what internal and external conflicts, these dissociative repressive mechanisms first manifest themselves. The consequences vary not only with the drives around which the conflict has arisen which gives rise to the repression, but also with the age at which the repression has occurred.

This unique aspect of the human neurosis (as of the human creative process) depends upon the interaction of the three types of symbolic function in the human being, which are generally known as the conscious, preconscious and unconscious systems. How these three interplay determines the fate of the creative process and of the neurotic process equally. They are always operating concurrently in varying degrees. Where conscious and preconscious processes play the preponderant role, patterns of behaviour are produced which are flexible, creative, responsive to external experience, satiable, and relatively free from automatic, rigid, stereotyped repetitiveness. On the other hand, where unconscious processes play a role which dominates over the conscious and preconscious symbolic processes, the resultant patterns of behaviour are rigid, insatiable, unalterable by external experience and rigidly repetitive. The critical question then arises, what at any moment determines the relative roles of the three symbolic systems? i.e., what psycho-physiological constants or variables in each human being? What inherited qualities? What experiential data? What are the genetic forces which originate this distribution and what forces can alter it? In the present state of our knowledge to be dogmatic in answering any of these questions is only to betray bias.

BIBLIOGRAPHY

1. BRICKNER, M., "Human cortical area producing repetitive phenomena when stimulated", *J. Neurophysiology*, 1940, 3, 125-130.
2. FISHER, CHARLES, "Dreams, images and perceptions: a study of unconscious-preconscious relationships", *J. Amer. Psychoanalytic Ass.*, 1956, 4, No. 1, 5-48.
3. KUBIE, LAWRENCE S., "A theoretical application to some neurological problems of the properties of excitation waves which move in closed circuits", *Brain*, 1930, 53, Part 2, 166-178.
4. *Idem*, "The neurotic potential and human adaptation", 1949, 77-96, from *Adaptation*, Romano, J. (ed.). Ithaca, New York: Cornell Univ. Press, p. 113.
5. *Idem*, "The neurotic potential, the neurotic process, and the neurotic state", *U.S. Armed Forces Med. J.*, January, 1951, 2, No. 1, 1-12.
6. *Idem*, "Some implications for psychoanalysis of modern concepts of the organization of the brain", *Psychoanal. Quart.*, 1953, 22, No. 1, 21-68.
7. *Idem*, "The concept of normality and neurosis", Chapter 1, pp. 3-14, from *Psychoanalysis and Social Work*, Heiman, M. (ed.), 1953. New York: Internat. Univ. Press, p. 346.
8. *Idem*, "Psychoanalysis as a basic science", pp. 120-144, from *Twenty Years of Psychoanalysis*, Alexander, F., and Ross, H. (eds.), 1953. New York: W. W. Norton, p. 309.
9. *Idem*, "Some unsolved problems of the scientific career", *Amer. Scientist*, October, 1953, 41, No. 4, 596-613; and January, 1954, 42, No. 1, 104-112.
10. *Idem*, "The Fundamental nature of the distinction between normality and neurosis", *Psychoanal. Quart.*, 1954, 23, No. 2, 167-204.
11. *Idem*, "An institute for basic research in psychiatry", *Bull. of the Menninger Clinic*, November, 1956, 20, No. 6, 281-287.
12. *Idem*, "The use of psychoanalysis as a research tool", *Psychiatric Research Reports* 6, *Amer. Psych. Ass.*, October, 1956, 112-136.
13. LEVIN, MAX, "Duplication, double orientation, and fusion", *Amer. J. Psych.*, August, 1957, 114, No. 2, 162-164.
14. LINDSLEY, DONALD B., "Basic perceptual processes and the EEG", *Psychiatric Research Reports* 6, *Amer. Psych. Ass.*, October, 1956, 161-171.
15. MARSH, JAMES T., and WORDEN, FREDERIC G., "Perceptual approaches to personality", *Psychiatric Research Reports* 6, *Amer. Psych. Ass.*, October, 1956, 171-177.
16. PENFIELD, WILDER, "Memory mechanisms". Presidential address before the Amer. Neurol. Assoc., June 1951, *Arch. of Neurol. and Psych.*, 1952, 67, No. 2, 178-198.
17. POETZL, O., "Experimentell-erregte Traumbilder in ihren Beziehungen zum indirekten Sehen", *Ztschr. f. Neurol. n. Psychiatrie*, 1917, 37, 278-349.
18. SPITZ, RENE A., "Hospitalism—an inquiry into the genesis of psychiatric conditions in early childhood", *The Psychoanalytic Study of the Child*, 1945, 1, 53-75. Internat. Univ. Press.
19. *Idem*, "Hospitalism—a follow-up report", *ibid.*, 1946, 2, 113-119.
20. *Idem*, "Anaclitic depression", *ibid.*, 1946, 2, 313-342.

DISCUSSION

By Dr. Emanuel Miller

It is with considerable regret that we have to receive Dr. Kubie's paper indirectly, but we are fortunate in listening to Dr. Strauss, who has read the paper with such grace and insight into the author's meaning. Having already studied Dr. Kubie's manuscript, I feel that his meaning has been admirably and accurately conveyed.

Those of us who have studied Dr. Kubie's earlier papers will recognize in today's communication that he has maintained what might be called his position—a point of view consistent over the years—of fidelity to both neurological and psychopathological concepts displayed against a background of philosophical standards or, rather, standards of scientific methodology.

I am largely in accord with Dr. Kubie's standpoint, having been subjected to the same scientific discipline of neurology and philosophy before descending to the valley of the shadows of psychopathology. For it is indeed a land of shadowy ideas compared with the agreed concepts of neurology and the demands of logic and method. Nevertheless, there is no need for the psychopathologist to apologize for the indeterminate nature of certain psychopathological formulations. The subject matter, being of the nature of history and, above all, the history of feelings, impulses, as well as of ideas, it must have vaguer outlines and contours than the data of the observable in the organic life, which are subject to the orderly scrutiny of sight, touch and the perceptual precision to which these can be ordered and categorized.

Still, Dr. Kubie's pleas to solve the conflict of organophobia and psychophobia must demand our attention and co-operation if we are to understand the nature of neurotic phenomena which he sees in the light of process. Dr. Kubie's paper, to repeat, is a logical consequence of his earlier contributions to psychiatric methodology, namely, his share in the Symposium on Brain and Consciousness and in the Hixon Symposium. To this train of ideas belong many other papers over 15 years.

This conception of process needs defining as it seems to carry a different connotation to philosophers and logicians from what it implies to medical diagnosticians. Whitehead, who gave much space to this notion in his writings, defined it as the growth and attainment of a final end. The progressive definition of the final end is the efficacious condition for its attainment. In that sense, all process is teleological—the fulfilment through history or experience of its fundamental organic strivings. What subsequently happens is the fittingness of those strivings in the world which allows for their realization.

To do justice to Dr. Kubie's standpoint we must follow carefully his line of thought and reasoning that is in the first instance what he implies by process. A process means the transformation of activity in any object or organism in contrast to its structure. It implies the manner in which the changes are effected. By implication, the procession of events is a destiny as if these were a foregone conclusion that such and such were the procession inevitably, given the initial condition. Kubie's analogy of Polio suggests that, given the infective agent which kills the anterior horn cells, the subsequent events are inevitable unless efforts are made to prevent them—this in itself implies foreknowledge of muscle shortening, ankylosis and certain consequences of bulbar paralysis. If he means *c'est le premier pas qui coute* then process is inextricably tied to destiny. In this sense the process is dependent upon the potential.

The neurotic process starts with a set of possibly specific causes which are augmented by tributary events which increase probably geometrically with feed-backs at each successive step. Taking average human histories, i.e., the cultural pattern of a given region or society, these augmenting features are likely to be recognizable and, therefore, the milestones on a neurotic unfolding are likely to follow along certain recognizable lines. The neurotic state is the cross section at any time, t_1 , t_2 , etc., if we care to examine the problem, or as it appears to the sufferer or indeed to the suffering observer, i.e., for example, a member of the neurotic's family.

As I see it, the relationship between potential and process is what modifies a potentiality given certain historical events or experiences which may precipitate anxiety, hysterical reaction, depression, obsessional patterning in place of depression (or even schizophrenic retreat). Names are labels for recognizing stations or the time of process unfolding—they are, as Dr. Kubie, I think, implies, demands, not new entities but transformations or emergents.

More picturesquely, it is the Freudian aphorism "Where Id was there Ego shall be". How the organism's process is realized will depend in some measure and inevitably upon the machinery, i.e., the nervous system and its metabolic gradients defined by the endocrine system, as much as upon the vicissitudes of experience. The latter will be a network of condition reflex patterns or conflicts which make up the Unconscious proper and the more flexible system or Preconscious on which Kubie, in my submission, justifiably places such emphasis. But, whereas Kubie confines the neurotic process to the true Unconscious—where reverberating cycles produce the repetition patterns and compulsions which are so typical of obstinate neurotic patterns—the Preconscious plays its part in giving flexibility and indeed imparting to neurosis particularly that semi-conscious quality inherent in such phenomena as the epinosic gain and variability of symptoms in obsessions, phobias and anxiety hysterical transformations.

In this sense, Kubie is justified in demanding that future enquiry will have to take account of two systems of variables—those deriving from the data of the neural-endocrine structures and their mechanics, and the psychic mechanism—the way in which events or history have got tangled in or supply the material for the neural machine. But I would add that, apart from actual organic disabilities demonstrable by tests, the psychopathologist has a right to pursue his enquiries and some of his therapeutic zest through psychic language alone.

For the purpose of discourse, there is no reason why the search for an overlapping language should not be developed and indeed encouraged. This might help Dr. Kubie's plea to understand those obstinate therapeutic hold-ups when the unexpected cure occurs, or when the unexpected intractable case damps our optimism.

The concept of the central emotional position, which is the basic reaction pattern of childhood, is, to my mind, not essentially different from the basic personality structure posited by certain American psychoanalytically oriented anthropologists. What gives it fixity—as if it were a photographic plate dipped in hypo—is still unknown. The genetic factor may prove a determinant, for it appears that only in certain mother-children relationships are certain patterns of satisfaction and deprivation assumed. In the majority, the process of maturation does not become arrested at an early fixation point. Here we are in danger of stereotyping the individual in a mould of theory. For example, a process may be seen only in the light of an analytically exposed process—this may be an analytic artefact repeated from patient to patient. Freud's analogy of the block of marble containing the ultimate statue suggests an imposition by theory.

Further, the basic emotional position may define the process within limits, but it is as capable of deviation as a mountain-stream which is deflected by the lie of the land and its strata which it will meet on the way to the sea. In other words, history must affect subsequent development although it carries some traces of earliest phases.

There are two inflexible factors—the initial process and the socio-ethical entanglements or conflicts of child-parent relationships which produce fixations. Nevertheless, as Dr. Kubie insists—and here I follow him—the Preconscious

gives evidence of, and allows for flexibility, certainly in the neurosis in which so much of the psycho-physical life is capable of normal response and variability under environmental pressure. In Kurt Lewin's terminology, it allows for wide field properties to develop within the organism. From one who is so committed to the Freudian concept of the Unconscious, it is welcome to hear that this region is not the sole determinant factor in the psychophysical life. I deliberately invoke the term psychophysical in this connection for the reason that the deepest pressures of the instinctual life must be understood in terms of physiology as well as psychology.

In confining to the true Unconscious the function of creating the patterns of neurosis (the consequence of unresolved conflicts and the tensions thereby set up) Dr. Kubie leaves to the Preconscious the function of outer adaptation in its own right, although it is geared to and influenced by the Unconscious. Its function is the sorting, discriminating, signalling and grading (cybernetic in the true sense) for the purpose not only of producing the high efficiency which the external world demands, but of overcoming the tendency to repetition which is the function of the lower mechanisms of the nervous system and which is exploited in the repetition character of all forms of neurosis. If the neurotic process was wholly bound by the primary emotional position, all the valuable acquisitions of the past life of the organism exclusive of the built-in patterns of constitution would not be available to give even the limited flexibility to the neurotic process. I presume that when Freud spoke of the transformation of Id impulses and unconscious conflicts into Ego processes, he meant to convey that the individual was thereby capable of unlearning and relearning.

If I might venture to extend Dr. Kubie's conception of Preconscious, it has many functions, not only giving the neurosis a degree of flexibility frequently ignored by clinicians and not exploited by them; it acts as a selective agent in creative activities, which, while drawing upon primitive or archaic images proper to the Unconscious, organizes them in a new fashion. In this sense we can the better understand the distinction not always drawn between neurotic and psychotic art and the art of the creative artist who selects, organizes and transcribes through the sorting apparatus of the Preconscious as well as by the conscious process of selection and arranging. To revert to the neurotic process, the phenomena of the secondary gain, which brings hysterical conduct so near to simulation, is made possible at the semi-permeable membrane between the Preconscious and the spearhead of full awareness.

The contribution of the Preconscious to the neurotic process, and its presentation to both sufferer and observer, will explain what we imply by maturity or maturation. This, by its very nature, implies a degree of freedom or flexibility which the neurotic pattern *per se* does not possess. Mature patterns are highly complex, their very complexity yields the possibility of modification with varied effort and observation well known to all learning experiments. Indeed at this level of complexity it is doubtful if neurological models will help us very much.

The analogy of the printing press comes to mind. If examined, the Hoe machine will tell all about typography, pagination and printers' errors, but nothing of the aspiration and prejudices of the leader writer.

Lastly, Dr. Kubie does not make himself wholly clear in the use of the term symbolic. Here he seems to have missed a great opportunity in conveying his deepest intention, namely, the distinction between archaic and higher level symbolic processes; the first manifested in the Unconscious and its neurotic derivatives, and the symbolic processes proper to the Preconscious and full

consciousness. The latter gives us verbal and mathematical symbols. Whilst the neurotic process may over time use verbal language, every analyst knows how its appearance betrays its distortions and primitive forms. The lower neural functions are of this order. The vigilance, as Head would call it, at the spinal level is of a different order from the vigilance at the highest levels of the neural structure. Translated into terms of neurosis, the analytical process is designed to make the highest level of vigilance master of the situation.

TREATMENT OF DEPRESSIONS

By

OSCAR DIETHELM, M.D.

It seems to me proper to stress from the beginning that there are various types of depressions. Often they are ill-defined, while at other times they fit into the clinical entities of manic-depressive reactions or later-life (involutional) groups. In addition, psychoneurotic depressions are also recognized. However, the essential nature of all these types of depressions is still poorly understood. Treatment therefore cannot be based on an aetiological concept or be specific for depression, but must be based on the psychopathological data.

In our current thinking we include in psychopathology the inseparably linked aspect of the overt and the covert. The overt data are directly obtainable and demonstrable and include symptoms and dynamic factors. The covert data include dynamic factors which cannot be recognized directly or may not even be demonstrable. Psychotherapy must be guided by current and past psychopathologic findings and the technical procedures must be adjusted to the psychopathological changes. When I proposed the concept of dynamic psychotherapy, I used the adjective "dynamic" because it emphasizes the moving force which is inherent in psychopathology and the active aspect of the therapeutic procedure. This concept demands that in treatment one pay attention to all factors which have a *dynamic* significance in the psychopathological reactions and in the healthy functioning of a person. These factors may be physiological, psychological, social or cultural. Their therapeutic significance depends on the constellation in relation to other factors, the period in the person's life during which they occur and the degree of their flexibility.

Clinical experience demonstrates the success with convulsive therapy in the middle and late-life depressions and in the depressions of the manic-depressive group. The results, however, are unsatisfactory in psychoneurotic and psychodynamically-determined depressions. It is also fully recognized that permanent results are often poor. Recurrences of the depressive illnesses result in prolonged incapacitations and painful experiences and frequently end in early death by suicide. At present the only hope for better-lasting results lies with psychotherapy, which may produce a healthier life adjustment and prevent recurrences. In the future, when a better understanding of possible physiological factors becomes available, physiological treatment may add to or replace some of the psychotherapeutic aspects which I am discussing.

Thus far, the results of the various analytic therapies have not been too encouraging, although in some patients valuable assets become established and psychological understanding permits the avoidance, or a better toleration, of the stresses of life. It has been stressed for many years that it is difficult to obtain a sufficiently far-reaching psychological understanding which would permit good results, because intense exacerbations of the depression with great suffering or suicidal danger prevent active therapeutic progress. Usually, after the depression has cleared, although the patient may participate therapeutically it is very difficult to reach him sufficiently.

Before outlining our method, I should like to mention that Freud referred to Griesinger's statement of 1875 that in the content of a psychosis the patient's

hidden dynamic factors become obvious. Freud related them very correctly to the same factors which he demonstrated in dreams.

In dynamic psychotherapy one must accept that there will be a constant change of theories with the progress of psychiatry and medicine in a constantly changing culture. It is important that the psychiatrist be aware of these influences and assume a critical attitude which will prevent him from seeking goals which are too limited or too far-reaching.

It is essential that one study the individual and his development in the world in which he lives. The significance of the dynamic events of childhood can often be recognized in the psychopathological manifestations of later life. It is therefore desirable that one investigate such dynamic factors which may relate to childhood in patients who are over 50 years of age as well as in the younger age groups. However, how much material should be elicited depends on the total psychopathological constellation.

Emotions may produce clinically and therapeutically disturbing symptoms. In people with cerebral arteriosclerosis for instance, anxiety may lead to an organic type of thinking disorder which may persist, but usually in one's therapeutic approach one succeeds in alleviating interfering anxiety. In depressions it is especially important that one should obtain the meaning of the emotions and establish their psychopathological influence and expression.

The therapeutic technique may vary, but usually an active form in several therapeutic sessions a week in which interpretations are used sparsely is the choice. Much repetition may prove to be undesirable and decentralization and desensitization may be effective tools. One must also emphasize usable assets and the need to tolerate that which cannot be changed within oneself or in the environment. The patient must learn to recognize that striving for certain life goals may lead to emotional difficulties because of an unchangeable discrepancy between his inadequacies and his desired goals.

The use of chlorpromazine has proved to be most beneficial in the treatment of depression because it alleviates intense anxiety, tension, resentment, anger, and transient sexual unrest. Through this decrease of the strength of the emotions, many thinking difficulties and depressive phenomena disappear and the patient is able to proceed with the dynamic analysis which may lead to an understanding of his personality and the factors which contributed to the illness and how these factors can be modified or changed. It is interesting to note the change in the subjective experience of depression without the above-mentioned secondary emotions. In these cases progress is rarely hampered by acute suicidal danger, but we think it essential that all these patients be considered potentially suicidal. This point is important because the usual signs which indicate to us decrease in suicidal danger in the depressed patients are hidden by the chlorpromazine.

I wish to outline briefly the treatment as we carry it out:

In depressions where intense emotions interfere with dynamic analysis, chlorpromazine is administered orally where there is intense anxiety and agitation in the amount of 400 to 800 mg. a day. In most cases 200 mg. a day are sufficient. Chlorpromazine is considered indicated if dynamic analysis is not possible in the acute phase of the depression. At the same time the patient receives the generally accepted psychotherapeutic aid in the form of a suitable routine in which work and appropriate recreation are balanced according to the patient's needs. Attention is paid to the physical needs, which include sleep, food, alcoholic beverages, smoking and drugs. Re-education is undertaken with regard to the faulty actions and behaviour, based on an understanding of the various dynamic

factors. Reassurance, based on signs of actual improvement, is utilized. Under the influence of chlorpromazine, which alleviates anxiety, guilt and resentment, and modifies agitation reactions, an attempt is made to analyse the patient's life development and learn the factors which played a role in the illness, as well as the assets which can be used in a constructive way. Termination of the chlorpromazine phase of treatment is indicated when the dynamic analysis has been completed satisfactorily. This evaluation depends upon the physician's judgment and experience. After termination, it occurs occasionally that a renewed phase of analysis under the influence of chlorpromazine might be indicated. If, after the essential psychotherapeutic goals have been reached, the depression has not improved greatly and apparently not close to recovery, the illness is terminated with electrically induced convulsions. This latter treatment has been necessary in the majority of depressions of middle and late life. Usually 5 to 7 convulsions, administered twice a week, were sufficient. In psychoneurotic depressions, dynamic analysis may proceed for a long period of time without chlorpromazine and may then occasionally be terminated by convulsions.

The final phase of psychotherapy is the dynamic formulation, which includes physical, psychological, social and cultural factors, and the attempt to consider the future life of the patient with the necessary adaptations. During the whole treatment, much attention is paid to the relationship with other members of the family who often may need a great deal of psychotherapeutic support if no actual aid.

The results have been most encouraging and seem to be superior to the termination of depressions by convulsive therapy without intensive psychotherapy. Recurrences have been infrequent. The danger is that the inexperienced psychiatrist will reach too soon for chlorpromazine without first determining whether this drug is actually indicated in the treatment of the depressed patient and to what purpose.

To summarize—the treatment of depressions must be guided by the psychopathological findings, and modified by the changes in the clinical picture. The goals of therapy should be primarily oriented to life adjustment—the patient developing self-reliance with an ability to adapt to inadequacies in himself which cannot be changed and to obstacles in the outer world which he cannot remove or overcome. Chlorpromazine may be a valuable aid in carrying out intensive psychotherapy while the patient is still deeply depressed. In this phase he can be reached more readily and the dynamic factors can be better recognized and investigated than in the convalescent phase of the depression. The organization of the patient's life during the illness, with desirable occupation, recreation and socialization is important and a valuable aid to a dynamically oriented psychotherapy. Convulsive therapy is of value in the termination of a slowly improving depression, especially in the ageing group and less in psychoneurotic depressions. With recognition of overt and covert psychopathological findings, with constant evaluation of psychological and physiological needs, with an appreciation of the family's reaction and of environmental and cultural factors the treatment of depression of any kind demands therapeutic activity, with stress on the psychotherapeutic aspects.

DISCUSSION

By **Prof. E. Stengel**, *Sheffield*

There must be few among us who did not know Professor Diethelm's work and who have not learned a great deal from his writings. He is one of the

distinguished psychiatrists of Swiss origin who have made such an impact on American psychiatry. As far as our discipline is concerned, the Mayflower set out from the shores of the Lake of Zurich.

In discussing the treatment of depressions Professor Diethelm has consistently refused to be impressed by authority and by preconceived ideas. He has looked afresh at what to many may appear old and unrewarding problems, and his presentation today has been singularly free from dogmatism. He made it, first of all, clear that in view of our ignorance about the aetiology of depression, treatment cannot be based on an aetiological concept but only on psychopathological data. This is a salutary reminder at a time when treatment of depression is so frequently coloured either by preconceptions about aetiology, organic or psychological, or else by therapeutic nihilism.

Professor Diethelm's observations about the limitations of electro-convulsive therapy are of special interest. The striking immediate effect of this treatment in many cases of depression has tended to obscure its limitations. Professor Diethelm's insistence on a combination of electro-convulsive treatment with psychotherapy is in keeping with his concept of psychodynamics, which embraces the effects of both physical and psychological treatment. I have been impressed by the fact that a course of electro-convulsive treatment often fails to have a satisfactory result in depressions when given early, while it may have a dramatic effect a few months later. It seems that long-standing depressions have what I would call a "refractory period", i.e. one in which the symptoms fail to respond to physical therapy. Obviously, a knowledge of this period would be of great value for the timing of treatment. I wonder whether there is not also a refractory period with regard to psychotherapy.

Professor Diethelm's observations on the role of anxiety in depressions and its effects on thinking, and his reference to the changing meaning of emotional disturbances are of great importance. Depression is all too often regarded as a simple and stereotyped condition. Another feature which is often taken for granted is the lack of sexual libido. Professor Diethelm has reminded us that sexual unrest is not infrequent.

Many psychotherapists have fought shy of the treatment of depressions, probably because it is for the therapist the most difficult mental disorder to tolerate and because frequently there is a "spontaneous" remission. Although we know nothing about the mechanism of the latter, not even whether it really is quite spontaneous, its frequency has nevertheless had a demoralizing effect on the psychotherapist's zeal. Professor Diethelm has not been influenced by these prejudices. He not only has told us that he has found psychotherapy useful but has also given us a clear picture of the kind of psychotherapy he has employed.

In the psychotherapy of depressions one soon comes up against the excessive sense of guilt, especially in the so-called endogenous depressions. The problem whether the profound guilt feelings so typical of these conditions are related to an emergence of pathological aggression which forms part of the illness, or whether they can be related to previously latent aggression, is still unsolved. This question is nevertheless of great importance in the psychotherapy of depressions. I gather that Professor Diethelm would discourage therapists from analysing these guilt feelings too actively during the depressive state, because such a procedure may deepen the depression and increase the suicidal risk.

Another example of Professor Diethelm's independence of majority opinion, which in clinical psychiatry has often proved mistaken, is his report on the effect of chlorpromazine in the treatment of depressions. Far from taking

it for granted that chlorpromazine, which has sometimes been known to cause depression, is therefore contraindicated in the treatment of depressive states, he has found this drug very helpful in certain cases. He has thus forced us to look again at this clinical problem, which too many of us have been inclined to regard as closed. In reconsidering this question we shall have to note the judicious and discriminating way in which he uses chlorpromazine, and its combination with psychotherapy and electro-convulsive treatment. The importance of a complete dynamic formulation considering all aetiological factors has been duly emphasized by Professor Diethelm.

The fact that chlorpromazine should be able on the one hand to produce depression as well as modify it, while also liable to produce a Parkinsonian syndrome, is of particular interest to me; for in 1937* I observed a recurrent syndrome consisting of typical depressive and Parkinsonian symptoms in patients with lesions of the brain stem. At the time I pointed out that this combination of symptoms was suggestive of certain types of depression being related to dysfunction of the brain stem. The fact that chlorpromazine and other tranquillizers have proved capable of producing both depressions and Parkinsonism seems to bear out this hypothesis.

These are only a few comments on Professor Diethelm's address which has been full of stimulating and important observations.

* Stengel, E., *Archiv. f. Psychiatrie*, 1937, 106, 726.

THE USE OF GENETICS IN PSYCHIATRY*

By

FRANZ J. KALLMANN, M.D.

It is good to be back in the serenity of this assembly after an interval of ten years, but I must ask your forbearance in view of the fact that the topic assigned me today is rather similar to the one I discussed here in 1947.

Psychiatry, as you know, abounds in fascinating subjects—dreams, sex differentiation hazards, philoprogenitiveness, and the like. Nevertheless, for many years now no one has asked me about my ideas on any of them. Instead, with a nod to the relentless wave of overspecialization in the modern human sciences, I shall re-evaluate the practical merits of genetic research data, especially those obtained in twin family investigations in relation to behaviour disorders.

As information contributed by *psychogenetics* to an understanding of normal and abnormal behaviour patterns is both direct and indirect, it would be well to appraise both types of evidence in the framework of what Braceland (7) recently called *the science of man*. It may be noted that in this context “psychogenetics” is used as a condensed term embracing the whole triad of physiological, psychological and psychiatric genetics.

Although various misconceptions persist regarding the proper place of genetics in the human sciences, there now seems to be a general willingness to concede that the experimental disciplines of medical psychology and psychogenetics meet on the common ground of their concern with the variable dynamics of human behaviour. The mutual interests of the two sciences lie in the very specific qualities that cause men to strive and create, to maintain health or succumb to adversity, to choose a proper mate, to work, reproduce and grow old, to die in harness or in the feeble shadows of retirement. There are biological foundations for each of these functions, and all of them are genetically controlled (16, 17).

While the chief objective of psychological research is to appraise quantitative and qualitative differences in traits, attitudes and personalities from an “ethnocentric” standpoint (10), the aim of genetic studies is to search for the *basic causes*. In line with psychogenetic concepts, the changing adjustment patterns of people and societies, as well as the various modes of adaptability or non-adaptability to a multitude of technical achievements of modern man, depend on determining basic factors, all of which are powered by genic elements.

No general theory of human behaviour nor causal model of specific abnormal behaviour patterns can be construed in a genetic vacuum, without some reference to primary gene action. Ethnoplasmic experiences which mould personality, and the formative elements which secure mouldability on the human level, are end-products of the same evolutionary process, and, like both sides of a coin, defy separation into independent variables (11, 16, 17).

Some of the underlying genetic principles are elementary, others highly complex. A few are still the subject of much hypothetical controversy, mainly

* From the Department of Medical Genetics of the New York State Psychiatric Institute, Columbia University, New York 32, N.Y.

because, as Gerard has stated, "the cleavage between natural and social science . . . is a cleavage between substance and action, body and soul, the objective and the subjective" (13). Another reason is that the science of human genetics is less than sixty years old and has been largely confined to the use of one quasi-experimental procedure—the *twin study method*.

While it is true that the complexity of human nature is such that it has prevented rapid advancement in the refinement of twin studies, this method has made it possible gradually to amass a wealth of empirical data, especially in regard to psychopathological variations. Proffering cohesion between the techniques of two co-ordinate scientific approaches—the psychological and the biological—the accumulated data call for certain generalizations which should condition the content of every psychiatric theory of fundamental intent.

Methodologically, it may be mentioned in passing that twin studies have been profitably applied in three different versions: (a) the twin-study method proper, (b) the co-twin-control method, and (c) the twin-family method. While all three versions are based on the regular occurrence of two genetically different types of twins—one-egg and two-egg—the main advantages of the third version lie in its effectiveness as a sampling method and in its inconspicuousness as a pluridisciplinary approach to total population surveys requiring combined cross-sectional and longitudinal investigations in a co-operative family setting.

The need for adequate *sampling procedures* is obvious if it is borne in mind that generalized conclusions should not be drawn from observations made in single pairs or in an unrepresentative series of pairs. The most reliable criteria for zygosity determination are dermatoglyphic and haematological data, occasionally to be supplemented by reciprocal skin grafts when the comparative scheme of the modern similarity method proves indecisive in differentiating same-sex twins.

Within the allotted time limit here, an account of psychiatrically pertinent findings, even if limited to data based on serial twin studies and rendered in telegraphic style, can only touch on a number of crucial issues. In the area of *normal personality variations* (15), gene-specific derivations have been shown to range from physical, co-ordinative, physiognomic and temperamental characteristics to intellectual abilities, affective regulations and special talents.

In between are sex maturation patterns, variations in antibody production and neurohormonal alarm reactions, the capacity for longevity, and the ingredients for sustained tolerance of physiologic or psychologic stress—a highly essential pre-requisite for a well-balanced personality. Except for one-egg twins, it is well known that each individual has his own threshold of adaptability to different types of stress, and his own pattern of stress symptom formation.

Consistent similarity in the composition of these personality components is not observed in the absence of genotypic identicalness. Two-egg twins of the same sex tend to differ as much in their personalities as any siblings reared together or apart. It is only in one-egg twins that even pronounced differences in life experiences, however adverse, are not potent enough to erase basic similarities in appearance and general personality traits.

The popular notion that the behaviour patterns of one-egg twins are alike chiefly because of unusual similarity in their early environments has yet to be substantiated. If confirmed, the argument would only strengthen rather than weaken any correctly formulated genetic theory. Psychodynamic concepts, too, are built on the premise that man is selective in respect to important aspects of his life experiences and so can be thought of as "creating his own environment" (1).

This principle is confirmed by the observation that a spiral-like development toward marked behavioural dissimilarity, such as has been reported for chronic alcoholism, delinquency or suicide, sometimes results from an apparently insignificant difference in the original adjustive patterns of one-egg twins (15, 16, 31). It is axiomatic, of course, that even the finest genetic endowment can go awry, either because of an unusual combination of adverse circumstances—intrinsic or extrinsic in origin—or because of prolonged abuse. However, extreme disparities of this type are the exception rather than the rule.

In evaluating the effect of genetic factors in *psychopathological variations*, it would be generally helpful to bear in mind that, from a genetic standpoint, unusual behaviour of any kind is viewed as an extremely complex and continuous chain of events in the individual's adaptive history, rather than the automatic expression of a fixed congenital error in metabolism. Accordingly, the division between a normal state of adjustment and those minor behaviour deviations commonly referred to as psychoneurotic cannot be regarded as static, nor as less vaguely defined than that between normal and subnormal intelligence.

As to *psychoneurotic* reaction potentials, Eysenck and Prell (12) were among the few who availed themselves of the opportunities afforded by the twin-study method. In line with their findings, they classified "the neurotic personality factor" as a biological and largely gene-specific entity, estimating the genetic contribution to this "neurotic unit predisposition" as 80 per cent.

Not so specific has been Slater's (30, 32) interpretation of neurotic symptoms as exaggerations of polygenically determined personality variants, less closely related to a given type of stress than to the basic personality. Despite "almost identical personality", seven of his one-egg pairs failed to present concordant psychoneurotic histories. Therefore, critical deviations in a person's history were assumed to be due to relatively chance occurrences, such as the personality of the chosen marital partner. In Slater's opinion, "one twin might suffer a mischance which would lead to a vicious circle of ill-health, social failure, hardship, discouragement and increased ill-health, while the other totally escaped". According to this theory, there are graded constitutional vulnerabilities in more than one dimension, so that "the man who breaks down with a neurotic illness is likely to be handicapped not by one constitutional weakness of severe degree, but with a number of minor weaknesses".

However, it may be noted that Shields' study (29) of same-sex pairs between the ages of 12 and 15 years provided evidence for one-egg twins being twice as likely as two-egg pairs to have the same degree of adjustive difficulty. With each child rated "on a four-point scale of psychiatric mal-adjustment", twins of either zygosity had no higher incidence of neurotic adjustment problems than single-born controls, but male twins and non-twins far exceeded their female counterparts in presenting some difficulty in adjustment. Only little more than one-half of the total group of school children investigated were classified as non-neurotic. Non-genetic explanations for the observed differences between one-egg and two-egg twins were rejected, somewhat summarily perhaps, especially in regard to the frequent similarities in type and severity of neurotic behaviour patterns.

One of the highest one-egg concordance rates reported has been for *homosexual behaviour* in the adult male (15), although all concordant twin partners in this series denied any mutuality in overt sex relations. Nevertheless,

44 one-egg pairs yielded a nearly perfect concordance rate, with the index cases standing at least midway on the homosexuality scale applied, and with pronounced similarity in the role taken by twin partners in their individual sex activities. In the two-egg group, nearly 60 per cent. of the co-twins of predominantly or exclusively homosexual index cases showed no evidence of overt homosexual behaviour at any age, and only 11.5 per cent. were given homosexuality ratings of five or six on Kinsey's scheme. The likeliest genetic explanation for these findings would seem to be a gene-controlled disarrangement in the balance between male and female maturation patterns, resulting in a shift toward an alternative minus variant in the integrative process of psychosexual maturation.

Another condition with a well-established one-egg concordance rate of close to 100 per cent. is an entirely different defect of more obvious organicity, namely, *mongolism*, the relationship of which to maternal age is regarded as fully substantiated (4). Since the corresponding rate for two-egg co-twins does not seem to exceed that of their later-born siblings (approximately 4 per cent.), the search for the aetiological factor in mongolism has been narrowed by twin data to a more or less permanent change in the mother's endocrine or reproductive system. Apparently, the noxious influence during a mongoloid pregnancy is not transient, but acts on a genetically predisposed embryo, or upon the ovum, or upon the embryo prior to the earliest stage when twinning occurs by division.

Serial twin studies have also aided in investigating the aetiology of two other organic syndromes, *cerebral palsy* (2, 34) and *convulsive disease* (9, 21). As to the former condition, Allen and Thums have indicated that twins are rarely concordant. There is a high rate of stillbirth or neonatal death in the co-twins of cerebral palsy cases, but no evidence for a specific genetic susceptibility to prenatal or natal injury. Apparently, many twins with cerebral palsy are survivors of adversities which proved fatal to their twin partners. The circumstances most likely to affect both twins of a pair include non-specific maternal and genetic factors, as well as prematurity *per se*, but probably not mechanical trauma during birth.

The most extensive analysis of epileptic twin pairs is that of Conrad (9), with concordance rates of 66.6 and 3.1 per cent., respectively. Unless clearly repudiated in the future, the results of this study represent strong evidence for the genetic origin of true convulsive disease.

According to Lennox and Jolly (21), cerebral dysrhythmia is an electroencephalographic expression of the epileptic genotype, assumed to be the result of a dominant gene by the Boston group, and of polygenic factors by Alström (5). In epileptic twins, 100 per cent. of one-egg pairs and 25 per cent. of two-egg pairs have been found to be equally dysrhythmic, despite marked dissimilarities in clinical symptoms.

In the area of *criminal behaviour*, there is still an emphatic need for well-planned cross-sectional and longitudinal twin data. Based on the findings of Kranz, Lange and others (19, 51, 30, 33), criminality concordance rates vary only from 14 per cent. in opposite-sex pairs to 54 and 66 per cent. in same-sex two-egg and one-egg pairs, respectively. This distribution indicates that both family milieu and basic personality traits play important parts in shaping the habitual criminal. Therefore, the trend toward similar criminal behaviour in two-egg pairs may stem largely from the effect of unfavourable environmental influences.

Measured by the same yardstick, concordance in one-egg pairs can often

be expected to extend to specific personality features likely to lead to a life of crime (brutality, ruthlessness, predatoriness, irresponsibility), rather than to the kind of crime perpetrated. With criminality itself determined in many cases by constellational factors, discordance may occur even in one-egg pairs, where one partner "manages to keep within the bounds of law, while the other having once taken a criminal step remains outside the law and does not find his way back" (31).

As to behaviour disorders that are not sufficiently explained on a situational or experiential basis, the list of conditions for which detailed twin data are now available is headed by the *schizophrenic* and *manic-depressive* types of psychosis (15, 16). Since these two disorders do not occur interchangeably in the same twin pairs, they are assumed to be genotypically specific. The potentialities for a cyclic psychosis are probably associated with a subtle disturbance in a neurohormonal control mechanism, which ordinarily protects a person from having harmful extremes of emotional responses. The concordance rates of two-egg and one-egg twins vary from 26.3 to 95.7 per cent.

While the tendency to exceed the normal range of mood vacillation apparently requires the mutative effect of a relatively dominant gene (with a tendency to incomplete penetrance), the metabolic deficiency in a potentially schizophrenic person seems to be the result of a basically recessive unit factor. Varying clinical expressions of the disordered behaviour pattern associated with the ensuing type of vulnerability to stressful experiences are probably produced by a number of modifying genes. Generally speaking, these variations depend on the type and degree of constitutional defence reactions that can be mobilized against the main biochemical (enzymatic) dysfunction. The concordance rates for two-egg and one-egg twins (based on a series of 953 pairs) are 14.5 and 86.2 per cent., respectively.

Regarding the morbidity risk for the offspring of matings between one schizophrenic and one non-schizophrenic parent, it is still correct to say that the expectancy of the disease approximates 16 per cent. and depends largely on the biological qualities of the second parent. However, the question of how to designate a mode of transmission that seems distinguished by a specific unit factor for the entire syndrome, plus a number of modifying genes responsible for the variable clinical expressions of the main genotype, has become more or less a matter of semantics.

Whether the given main gene seems to be primarily dominant or recessive, or fails altogether to show simple segregation ratios, the modern concept of Mendelian heredity implies equivalence with chromosomal heredity. At the present stage of knowledge, one no longer believes that the demonstration of a gene-specific mechanism "hinges upon the counting of simple Mendelian ratios" (3). Hence the classifications of dominance and recessiveness are now regarded by some as no more than "operational definitions for counselling purposes" (14).

Apparently, most pathological conditions in man are neither fully dominant nor completely recessive, but only relatively so, depending upon the readiness with which the trait is detectable in the heterozygote. It has even been suggested that all carriers of mutant genes would react abnormally if put under sufficient biological stress. What can certainly be stated with assurance at this point is that regardless of whether the effect of a mutant gene expresses itself as an enzymatic disturbance or a lack of ability to respond to certain environmental stimuli with a normal type of behaviour, the potentials of a disordered behaviour pattern are likely to stem more directly from a change in primary gene action than can be assumed for a correlated anatomical defect (3).

As to the *basic metabolic deficiency* that may produce the specific potentials of a schizophrenic reaction pattern, it has recently been suggested by the Alexander-Funkenstein group, Wooley and Shaw, the research teams of Hoffer, Jungeblut, and Leach and Heath, and only a few months ago by the DPP oxidation studies of Akerfeldt (N,N dimethyl-p-phenylenediamine) that the given vulnerability component may be correlated with one of the following factors: altered reactivity to autonomic drugs, especially mecholyl; or changes in the production of a hypothetical anti-epinephrine factor or of some other constituent of a faulty epinephrine metabolism, including serotonin and the oxidized adrenaline derivatives adrenolutin and adrenochrome which are related chemically to such substances as mescaline and lysergic acid; or changes in the functional properties of certain copper-containing or receptor-destroying enzymes that are essential in the control of blood plasma components, such as ceruloplasmin, or of the surface activities of red cells. One of the earliest working hypotheses assumed a genetically determined dysfunction of cellular elements, derived from the reticulo-endothelial system, and such a possibility has yet to be ruled out by adequate studies.

On the whole, therefore, it must be stated with regret that despite considerable progress in the cellular, metabolic and biochemical areas of personality organization, the primary physiodynamic substrate of a schizophrenic psychosis remains veiled in mystery. From a genetic standpoint, it seems advisable to add a word of caution with respect to the current tendency to assume a gene-specific variation whenever some anatomical or chemical phenomenon is found in a population showing the symptoms of a behavioural abnormality. With each new report of a possible organic correlate of schizophrenia, we would do well to remember that should one of these findings stand up under scrutiny it may be the consequence of a patient's disturbed behaviour, rather than the gene-specific cause.

Involucional melancholia and other non-periodic forms of depressive behaviour in the involucional and senile periods have been shown by twin data to be unrelated to the manic-depressive group of disorders. There is an indirect link with the schizophrenic genotype through certain forms of emotional instability characteristic of schizoid personality traits. Other symptoms of maladjustment in the senescent period may arise either from gene-specific metabolic dysfunctions peculiar to the senium, or from graded differences in general health and survival values. Again, it is regrettable that little is known about the gene-controlled biochemical disturbance even in such well-defined clinical entities as cerebral arteriosclerosis, Huntington's chorea, and the pre-senile brain atrophies of the Pick or Alzheimer variety. With all these disorders, beyond demonstrating that hereditary elements play an essential part in the aetiology, the ideal goal would be to show precisely how this action takes place.

Even if the search for definitive biochemical correlates of gene-specific deficiency states is not too fruitful in the immediate future, the potential value of successful progress in this direction cannot possibly be overestimated. With all available facts pointing to complex modes of causation in most psychiatric disorders, it is clear that the prospects are dim for conquering such illnesses without well-planned attacks on the cellular and molecular levels.

Reluctance to recognize the operation of genic elements in the aetiology of psychoses, such as schizophrenia, is certain to have harmful consequences in a number of respects. The common denominator for damaging trends of this kind may be seen in the toleration or promotion of general attitudes that are liable to divert attention from important genetic issues.

Beyond doubt, the tendency to de-emphasize genetic factors as potential causes of behaviour disorders is an occupational hazard of our profession and older than psychiatry itself as a social phenomenon. Whether accomplished by fiat or the Promethean power of allegoric expression, this tendency easily grows into a habit. While it may be obcurable by fluid levels of abstraction, it is sufficiently potent to deflect much investigative and therapeutic effort. Neither conceptually nor for purposes of guidance can the resultant attitude be expected to be of any more help than the affirmation of a fruitless daylight search for owls in Athens or for coal pits in the heart of Newcastle.

Aside from harmful omissions in treatment and family guidance, damage may be caused by disregard of two general genetic phenomena: (a) the hazards of a gradual increase in the number of poorly adapted carriers with a chance of reproduction, such as may result from assortative mating trends and improved medical techniques; and (b) further increases in the mutation rate of populations due to excessive use of ionizing radiation.

The theory that an increasing *load of mutations* in future generations will be the penalty for relaxing natural selection by medical advances has been supported by many genetic experts (25, 26). It is based on the estimate that the average person is heterozygous for at least eight genes of a specified variety. Under the conditions of equilibrium prevailing for present gene frequencies, there would be an effective genetic loss in the population of 20 per cent. resulting from the total of these "more or less familial patterns of weaknesses".

With a large part of this segment apt to be saved for reproduction by modern medical procedures, the frequency of mutant genes will inevitably rise to a new level. Accentuated by the accumulation of gene-specific disabilities due to ionizing radiation, the adverse consequences for future generations may reach a point where the population's survival is threatened.

In view of this danger, *genetic radiation hazards* have become a matter of serious concern to everyone in recent years, including psychiatrists and neurologists and any other profession requiring extensive medical uses of X-rays. Only last year, sorely needed precautions against exposure to mutagenic energy sources were studied by special committees here in England (24) as well as in the United States (8). The prevailing view was that because of the cumulativeness of genetic damage caused by radiation, the average person ought not to receive more than 10 roentgens of man-made radiation up to age 30, and preferably less. The recommendations made by the committees included general public health measures designed to limit medical irradiation to safe lifetime amounts and to the lowest doses consistent with medical necessity, and the organizations of much more systematic research "about all kinds and all levels of genetics".

In this kind of programme, which obviously should be extended to the aetiology of all psychiatric disorders of partly genetic origin, twin family studies are certain to contribute important information. Fortunately, various well-equipped laboratories are now working on these problems, and there is hope that they may soon come up with some penetrating clues.

In addition, what is needed perhaps as urgently as increased genetic knowledge is the foresight implicit in a concern with the health of future generations. Real progress in this direction will depend on our present willingness to exercise such voluntary restraints as are recommended by public health authorities, and on the promise of concerted action on the part of sciences dealing with the health prospects of families and populations.

BIBLIOGRAPHY

1. ALEXANDER, F., "Psychoanalysis in Western culture", *Amer. J. Psychiat.*, 1956, **112**, 692-699.
2. ALLEN, G., "Cases of cerebral palsy in a series of mentally defective twins", *Amer. J. Ment. Def.*, 1955, **59**, 629-639.
3. *Idem*, "Genetic aspects of medical disorders", in "The Nature and Transmission of the Genetic and Cultural Characteristics of Human Populations", *Proc. 1956 Amer. Conf. Milbank Memorial Fund*, 1957. New York.
4. *Idem* and BAROFF, G. S., "Mongoloid twins and their siblings", *Acta. Genet.*, 1955, **5**, 294-326.
5. ALSTRÖM, C. H., *A Study of Epilepsy in its Clinical, Social and Genetic Aspects*, 1950. Copenhagen: Munksgaard.
6. ALTSHULER, K. Z., "Genetic elements in schizophrenia", *Eugen. Quart.*, 1957, **4**, 92-98.
7. BRACELAND, F. J., "Psychiatry and the science of man", *Amer. J. Psychiat.*, 1957, **114**, 1.
8. COMMITTEE ON GENETIC EFFECTS OF ATOMIC RADIATION, "The Biological Effects of Atomic Radiation", 1957. Washington, D.C.
9. CONRAD, C., "Die erbliche Fallsucht", in *Handbuch der Erbkrankheiten* (A. Guett, ed.), 1940, **3**, Part 2. Leipzig: G. Thieme.
10. DAVID, H. P., and V. BRACKEN, H., *Perspectives in Personality Theory*, 1957. New York: Basic Books.
11. DOBZHANSKY, T., *Evolution, Genetics, and Man*, 1955. New York: J. Wiley & Sons.
12. EYSENCK, H. J., and PRELL, D. B., "The inheritance of neuroticism: an experimental study", *J. Ment. Sci.*, 1951, **97**, 441-465.
13. GERARD, R. W., "Units and concepts of biology", *Science*, 1957, **125**, 429-433.
14. HSIA, D. Y., "The laboratory detection of heterozygotes", *Amer. J. Hum. Genet.*, 1957, **9**, 98-116.
15. KALLMANN, F. J., *Heredity in Health and Mental Disorder*, 1953. New York: W. W. Norton & Co.
16. *Idem*, "The genetics of human behavior", *Amer. J. Psychiat.*, 1956, **113**, 496-501.
17. *Idem*, "Psychogenetic studies of twins", in *Study of the Status and Development of Psychiatry in the United States* (S. Koch, ed.), New York: McGraw Hill. In press.
18. *Idem* and RAINER, J. D., "Genetics and demography", in *Demography as a Science* (T. M. Hauser and O. D. Duncan, eds.), Chicago: Population Research and Training Center, The University of Chicago. In press.
19. KRANZ, H., *Lebensschicksale krimineller Zwillinge*, 1936. Berlin: Springer.
20. LANGE, J., *Verbrechen als Schicksal*, 1929. Leipzig: G. Thieme.
21. LENNOX, W. G., and JOLLY, D. H., "Seizures, brain waves and intelligence scores of epileptic twins", in *Genetics and the Inheritance of Integrated Neurological and Psychiatric Patterns* (D. Hooker and C. C. Hare, eds.), 1954. Baltimore: Williams & Wilkins Co.
22. LUXENBURGER, H., "Psychiatrisch-neurologische Zwillingspathologie", *Zbl. ges. Neurol. Psychiat.*, 1930, **56**, 145-180.
23. *Idem*, "Untersuchungen an schizophrenen Zwillingen und ihren Geschwistern zur Prüfung der Realität von Manifestationsschwankungen", *Z. Neurol.*, 1935, **154**, 351-394.
24. MEDICAL RESEARCH COUNCIL, *The Hazards to Man of Nuclear and Allied Radiation*, 1956. London: Her Majesty's Stationery Office.
25. MULLER, H. J., "Our load of mutations", *Amer. J. Hum. Genet.*, 1950, **2**, 111-176.
26. NEEL, J. V., "The study of human mutation rates", *Amer. Naturalist*, 1952, **86**, 129-144.
27. PLANANSKY, K., "Heredity in schizophrenia", *J. Nerv. Ment. Dis.*, 1955, **122**, 121-142.
28. RADO, S., "Evolutionary basis of sexual adaptation", *J. Nerv. Ment. Dis.*, 1953, **121**, 389-396.
29. SHIELDS, J., "Personality differences and neurotic traits in normal twin school children: a study in psychiatric genetics", *Eugen. Rev.*, 1953, **45**, 213-246.
30. SLATER, E., *An Investigation into Psychotic and Neurotic Twins*, 1951. London: University of London.
31. *Idem*, *Psychotic and Neurotic Illnesses in Twins*. Part II. Medical Research Council Special Report Series No. 278, 1953. London: Her Majesty's Stationery Office.
32. *Idem*, "Psychiatry", in *Clinical Genetics* (A. Sorsby, ed.), 1953. St. Louis: C. V. Mosby.
33. STUMPL, F., *Die Ursprünge des Verbrechens, dargestellt am Lebenslauf von Zwillingen*, 1936. Leipzig: G. Thieme.
34. THUMS, K., "Die angeborene zerebrale Kinderlähmung", *Allg. Z. Psychiat.*, 1939, **112**, 262-293.

DISCUSSION

By Dr. Eliot Slater

A few days ago I was having the interesting experience of reading the 8-page report of an American colleague on a patient of his, a millionaire's daughter,

who had been having psychoanalysis for over 5 years, at a cost, in her maintenance in the U.S.A. and her treatment, of about 150,000 dollars. Perhaps not unnaturally, her father was wanting some information on what the treatment had been. Now it was clear from the report, first that the patient was what we should call a hysterical psychopath of an exceedingly difficult type, and secondly that the psychiatrist was a man of knowledge and ability, had a very thorough understanding of the case, and had treated his patient conscientiously and with quite a measure of success. In his report to the family doctor, which would be discussed with the girl's father, he drew attention to her immaturity and egocentricity, the ways in which her deficits of personality had affected her behaviour and altered her life, and what had been and had still to be done about it. I felt that he and I, if we had met in consultation, would have had no difficulty in understanding one another. Yet, in a letter following on this report, quite a short letter giving a formal diagnosis for the benefit of any consultant who would be later brought in, my American colleague gave it as his opinion that the patient was basically psychotic and was probably schizophrenic.

There was no escape from the view that these words must mean something very different to him and to me. What I should mean by psychotic would be suffering from one of the psychoses, such as a manic-depressive state or a schizophrenia, without anything being implied about its gravity, or prognosis, or treatment. I suspect that, for the American, the word psychotic meant something very different, such as lacking in insight, or fundamentally unalterable, or with a core of abnormality going beyond the reach of psychological explanation. I suspect, also, that when he used the word schizophrenic, which means so much to me, for him it added very little or nothing to what he had already said.

This simple illustration exemplifies the great weakness of psychiatry today. We are using words which have different meanings for some of us from what they have for others; and I think one can also say that sometimes we use in a loose way words which should only be employed precisely. Such a word is schizophrenia.

Many of you will think this is altogether too bold a claim. Yet I believe it is firmly based, on solid evidence, a great deal of which has been provided by Professor Kallmann. It is time that this evidence was examined in an unbiased way, and if it does not fit in with our preconceptions, then our ideas should be altered to make room for it. Professor Kallmann says, that in the enormous number of twins he has examined, he has found no single uniovular pair in which one twin suffered from schizophrenia and the other from an endogenous affective psychosis. Yet we still have teachers of psychiatry in this country who go around saying that there is no qualitative difference between schizophrenia and the affective psychoses, but that they shade off into one another, and are only the opposite extremes of the schizo-affective range. Surely those who take this view are wearing blinkers, and have not troubled themselves to examine the evidence that genetical research brings to bear on the question.

There is another similar piece of genetical evidence of crucial importance. It is sometimes claimed that the causes of schizophrenia in the adult are to be found in the psychological environment of the young child, that the relationship with the mother is invariably disturbed, and that the mother who predisposes her child to the later development of a schizophrenia is one who is herself schizoid, cold, affectionless. Now the fact of the matter is that, in this particular connection, the mother seems to occupy no more important a place than the father. There is a raised incidence of schizophrenics and the fathers

are equally likely to be schizophrenic themselves, or, for the matter of that, schizoid. This has been shown by Manfred Bleuler, who is an enthusiastic protagonist of the importance of psychogenesis in schizophrenia.

I do not wish to suggest that these genetical facts should come between the clinician and his patient, and change his attitude to treatment, e.g. by making him throw up his hands in despair and say that where genetical causes are involved there is nothing to be done. Professor Kallmann has emphasized that, for psychiatry, genetics is a basic science. It is a means of exploring basic causes. The knowledge it provides will have important effects on one's understanding of psychiatry. It could and should affect one's ideas on classification, on promising methods of research, on the nature of the problems we face. But it cannot have much effect on one's understanding of the individual patient. Boyle's law is a basic part of the knowledge required by the heating engineer; but it does not help very much when we come to try and find out why it is that our domestic boiler has broken down. The understanding of what happens when foodstuffs are cooked depends, in a fundamental way, on the solution of subtle and difficult problems of chemistry. By research of that kind we can explain how it is that poor cooking may cause a risk of vitamin deprivation. But however much chemistry we know, we shall not be helped when we come to consider how it was that we spoiled that omelette. Now we psychiatrists are much more like boilermen and cooks than we are like physicists and chemists. It is right that we should devote our first attention to practical matters of every-day life, involving individual patients. But we shall be fools if we neglect to equip ourselves with some knowledge of basic theory; and we shall be bigots if we refuse to accept such fragments of knowledge as we can get, wherever we get them. It is a reproach to be told that "the tendency to de-emphasize genetic factors as potential causes of behaviour disorder is an occupational risk of our profession". We must not cling to the mantle of a single prophet. When we come to a case of epileptic psychosis, neither Pavlov nor Freud are very much help.

I have already mentioned a common error, namely the idea that even an admission of genetic causation as a part factor in psychiatric illness will paralyse therapeutic activity. So far is this from being true, that its converse should be expected. Exploration of genetic causation may even supply us with new and unexpected methods of treatment. I should like to refer to the work of Roger Williams on alcoholism in rats. He found that it was quite easy to breed strains of rats which, as a result of their genetic constitution, readily became alcoholic addicts. The animals would take up to .6 of a millilitre of alcohol per 100 gm. daily, the equivalent of half a litre of pure alcohol for an adult human being. Nevertheless, these rats could be successfully treated for their addiction. For instance in one strain of addicts it was found that the provision of abundant supplies of vitamin A abolished the alcoholism, and in other strains other dietary deficiencies seemed to be operative. Williams has developed the interesting concept of genetotrophic diseases. Every individual that has a distinctive genetic make-up has distinctive nutritional needs, which must be met for his well-being. A consequence is that different individuals can support different degrees of deprivation of essential nutrients before their health is impaired. In a relatively uniform environment, some will and some will not fall sick. The difference between the one who does and the one who does not is genetical in nature, but the adequate treatment will be environmental.

Before I close I should like to pay tribute to Professor Kallmann's absorbingly interesting address, but still more to pay tribute to his studies, of

fundamental importance, which have penetrated so many fields: schizophrenia and the other endogenous psychoses, ageing and the organic psychoses, homosexuality, and mental deficiency, to mention only some of the most striking. We are under a lasting debt to him for his work, and for being so good as to come here and tell us about the field in which he is a pioneer, when he might so much more profitably to himself have been enjoying the sunshine in Switzerland.

PHYSICAL TREATMENTS IN PSYCHIATRY

By

LOTHAR B. KALINOWSKY, M.D.

New York

It has been stated too frequently that new therapeutic developments in psychiatry are revolutionizing the field. While this did not turn out to be true for any of the treatment procedures, it can well be stated that those of us who entered psychiatry more than 25 years ago, lived through a period in which psychiatry changed from a specialty known for its therapeutic nihilism to a field of almost feverish therapeutic activity. Undoubtedly the physical treatments have played a decisive role in this change. They have also clearly changed the attitude on the part of the individual psychiatrist to his specialty. Our teachers were primarily concerned with attempts to organize and systematize our knowledge, while our generation, perhaps because too pragmatic, may easily be blamed for failure to link the newly-acquired information with the basic concepts developed by them, concepts which were neither confirmed nor disproved by our new knowledge. When I think of my own development as a neuropsychiatrist, I remember that I started out in psychiatry, but temporarily turned to neurology because at that time neurology seemed to give much greater therapeutic possibilities. It was only in the middle nineteen-thirties that this changed radically, and that so many more therapeutic possibilities developed in psychiatry that it became a challenge to return to this field. The introduction of the various physical treatments in close succession was an amazing coincidence. Although met by some with scepticism and even ridicule, and by others with unjustified over-enthusiasm or a competitive attitude improper for scientific progress, the new treatments evoked a beneficial optimism among psychiatrists. They helped to change the attitude of the mental hospitals in all countries and gave an entirely new impetus to the general public to become interested in matters concerning the mentally sick. It may also be said that the physical treatments gave a new stimulus to efforts at psychological and social treatments of the psychotic patient, and I dare say that the improvements in mental hospital management which are so conspicuous in your country could not have progressed so fast and met with so much public support if the physical treatments had not helped to improve the adaptability of patients in these hospitals. It is a historical fact which can be easily proved, especially in England, that those hospitals which are now the showplaces for modern hospital treatment, with open wards and social and recreational activities for almost every single patient, are also the ones in which pioneer work was done with insulin coma treatment, convulsive treatments and psychosurgery. I am going to discuss these treatments, while the newest approach, pharmacotherapy, will be left to another report and be mentioned only as far as it influences the application and indications of the other treatments.

Such a paper gives a good opportunity to take stock of the present status of the various physical treatments and to re-evaluate some of the inherent clinical and theoretical problems. Such a survey will by-pass the generally known and accepted facts and concentrate on the many debated questions. There are all too many problems still under debate, and it is regrettable that

the newer and more publicized pharmacological developments have diminished efforts to discuss and to clarify the unanswered questions set by the preceding physical treatments.

Insulin coma treatment is a good example for this disregard of previous treatments. There has been hardly any literature on insulin coma treatment during the last few years. The appearance of chlorpromazine and reserpine led prominent psychiatrists without practical bedside experience to statements that the shock treatments were a thing of the past. This was welcomed by some hospital administrators who had always found insulin coma treatment a great inconvenience. While in our own state hospitals in New York insulin units have never been discontinued, other hospitals, including private ones, did give it up. Already once before the same trend has been observed when electroshock was introduced into hospitals whose insulin units had been hampered anyway by the war. It is a definite sign in favour of the efficacy of insulin treatment that it survived, and that a recent symposium in Zurich clearly demonstrated that experienced clinicians still feel the need for it. Its technique is practically unchanged since its introduction by Sakel in the Vienna clinic. Modifications such as the one by Shurley, who doubled the dose with each subsequent treatment until he reached coma level, or reduction of the amount by giving amorphous insulin or adding hyaluronidase have not yet been generally accepted. The dangers of protracted coma have never been entirely overcome, and even the best set-up cannot avoid an occasional fatality from cardiovascular collapse or protracted coma. The greatest handicap for the therapeutic effectiveness of insulin treatment continues to be fear on the part of the physician of producing deep coma. Many unfavourable statistics on results of insulin treatment are obviously explained by technical shortcomings in a treatment whose effective agent is apparently not the insulin as such but the coma produced by it. The light hypoglycaemia found to be useful in neurotic syndromes has no effect on the schizophrenic psychosis. Depth of coma and number of comas are the decisive factors, contrary to some statistics which seem to show that best results are obtained with short courses of insulin, namely in patients who have a good prognosis in the first place. Unfavourable results in spite of large numbers of comas are explained by the treatment of chronic cases, the failure to respond being in spite of, not because of, the large numbers of comas given.

This is not the place to discuss technical details, important as they may be. Whatever has been said about the need for highly qualified personnel in an insulin unit is as true as ever, if one is to avoid unnecessarily high numbers of protracted comas and other accidents. The best treatment for protracted coma still consists in its prevention, although some reports on anaesthesia techniques to keep the patient oxygenated seem to be more promising than other recommendations.

Is it worth while to maintain such a complicated, expensive and often dangerous treatment? The answer must be "yes" considering the seriousness and the therapeutic resistiveness of the disease schizophrenia for which insulin coma treatment is applied. Bourne wrote a rather sensational article entitled "The Insulin Myth", in which he stated that electric shock therapy can achieve just as much as insulin treatment. Although I agree with many of his statements, and although I do not fully agree with Sargant and many others that insulin should be given preference over electric shock in every schizophrenic, my personal experience has clearly convinced me again and again that many failures of even repeated electric shock series may still respond to insulin coma

treatment. This is not in contrast to the experience that many schizophrenics who respond to any treatment have good and lasting remissions after electric shock alone. I, therefore, maintain that electric shock, which is easier to apply and certainly less dangerous than insulin, should be given first choice in schizophrenia, and this includes paranoid cases as well as catatonics. A difference in indications for insulin and E.C.T. according to subtypes of schizophrenia has been stated, but it has never been possible to prove this. It is true only in so far as the less favourable types like the paranoid, with its usually later and more insidious onset, show less response to any treatment, and therefore, here electric shock treatment will have to be followed more often by additional insulin coma treatment. I have no argument with those who prefer to give insulin in the first place in these cases with less favourable prognosis, but for the sake of clarity and in view of the limitations of insulin beds in most psychiatric hospitals insulin should not be considered the only treatment for schizophrenia. To what extent the indications for both forms of shock therapy have changed with the introduction of the newer drugs will be discussed later.

There is much evidence that electric convulsive treatment is still widely applied in all psychiatric hospitals. Of course, in chronic schizophrenia pharmacotherapy has practically abolished the indications for electric shock therapy which had never been too satisfactory, and this may account for a statistically impressive reduction of the number of treatments given in some of the larger mental institutions. In hospitals with more acute material its use continues to be extensive if it is not restricted by a general policy on the part of the management of the hospital. This was clearly evidenced by a recent poll among psychiatrists with special experience in shock treatments, and it was equally evidenced in the U.S. by figures from most private psychiatric hospitals. As an example, at the psychiatric pavilion of a general hospital, St. Vincent's in New York, where during the first five months of this year 278 patients had been admitted, 136, or half of the total number, received E.C.T.

Contrary to insulin coma treatment, electric shock underwent much experimentation in the hope to overcome some complicating factors. It may be mentioned here that pharmacologically induced convulsions with metrazol or similar drugs are still used in combined insulin-convulsive treatment by some. The slight superiority of metrazol in the treatment of acute psychotic states such as catatonic excitement still leads some psychiatrists to give it preference and the introduction of succinylcholine-pentothal anaesthesia, which spares the patient the unpleasant experience of metrazol, has revived interest in this type of therapy. However, this revival by Komora and Padula and others has not yet achieved great significance. It may be added, though, that in one of our State Hospitals in Maryland Kurland has introduced an inhalant convulsant drug discovered by Krantz and named indoclon (hexafluorodiethyl) in the hope of being able to avoid some of the side-effects of the electric current, such as memory impairment and fractures. This new method appears to me promising, because the patient is already unconscious and relaxed when after some irregular contractions he enters the tonic phase of the convulsion.

The side-effects attributed to the electric current, have led to constant attempts at applying different types of current. Although this kind of research initiated in the United States and is constantly being stressed by some manufacturers, it can be stated that it is just in the U.S.A. that most psychiatrists still use, and in many cases have gone back to using, the alternating current recommended by Cerletti and Bini, while in other countries, as, for instance, in Germany, the most widely used standard machine applies different currents.

Progress in this field has been hindered by the presentation of unproven statements rather than comparative statistics when new methods have been introduced. Another hindrance was the fact that the newer techniques apply smaller current intensities for a longer period of time, and this could be—but never has been—done just as well with the alternating current. Evaluation of such techniques as the Reiter is further complicated by the fact that here the current is maintained during the whole convulsion in an effort to suppress the clonic phase. There is some evidence that such continued stimulation, already recommended in the so-called electronarcosis treatment by workers from California, may be more dangerous for the patient. Memory impairment seems to be somewhat less pronounced with the newer techniques, which may be explained by the lesser intensity of the current. The second reason given in favour of these modifications, the prevention of fractures, could not be maintained. It can be noted that some authors who used these modified currents were the first to recommend muscle-relaxant drugs for the prevention of fractures.

Premedication with muscle-relaxant drugs is at present the main issue in electric shock therapy. It is true that medico-legal considerations have pushed more and more psychiatrists into the application of muscle relaxants. It is equally true that particularly those with large experience in unpremedicated electric shock are deeply convinced that the danger of E.C.T. to life and the number of actual fatalities become greater with any type of premedication. Although prevention of fractures is always achieved with the muscle-relaxant drugs, I cannot agree with the statement that they diminish the danger of cardiovascular complications. As logical as this may seem, clinical evidence definitely contradicts this assumption. I have seen several cardiac patients who were able to stand unpremedicated electric shock in previous episodes without any trouble, but when started on succinyl-choline and pentothal premedication went into a collapse-like condition. When treatment was continued without premedication, the patients again did not show any distress. It is true that the high fatality rate of curare and similar drugs in former times does not apply to succinylcholine. There remains the fact, however, that a convulsion as such, as is well known from the literature on epilepsy, hardly affects even patients in poor physical condition and suffering pre-existing physical disease, and the same is true for electrically induced convulsions as evidenced by the experience with E.C.T. prior to the introduction of muscle-relaxant drugs. A recently reported case of a patient who after many unpremedicated electric shocks died in the first treatment given with pentothal premedication without succinyl-choline suggests that the pentothal rather than the succinyl-choline or the combination of the two is responsible for untoward results in some instances. Therefore some psychiatrists use a technique, first published by Impastato, in which succinyl-choline is given first, and instead of pentothal an immediate subconvulsive stimulus is used to make the patient unconscious and unaware of the suffocating feeling of the succinyl-choline injection. This subconvulsive stimulus is then followed 30 or 45 seconds later by the convulsive stimulus, when muscle relaxation has set in. One advantage of this technique is that much smaller amounts of succinyl-choline, often less than 10 mg. are sufficient for complete muscle relaxation. In my experience, a disadvantage of this method is that the current used for the first stimulus is often either too low and, therefore, felt by the patient, or too high, thus leading to a convulsion before the succinyl-choline has had a chance to act. Also post-treatment discomfort and excitements seem to be more frequent without the pentothal. In all treatments

with succinyl-choline I insist on the presence of an anaesthetist. Where pentothal is not contraindicated because of cardiac or pulmonary conditions we use the drip method for the slow introduction of pentothal and give the succinyl-choline into the tubing, which is left in after the treatment for eventual emergency measures, a technique used by the anaesthetist Dell'Aria in my own clinical material. However, I avoid premedication wherever possible, as, for instance, in young females and in patients who have had previous E.C.T. without complications and in whom bone complications need not be expected.

I may add that I also avoid premedication with barbiturates alone wherever possible.

A discussion of psychosurgery is easier and less likely to meet with opposition in England than elsewhere. In spite of Walter Freeman's pioneer work in the U.S.A., psychosurgery soon became a more widely used procedure here than in America. Yet, in the U.S. also frontal lobe operations found a broader application than could be expected considering the psychodynamic approach of American psychiatry. It certainly became a more widely accepted procedure in the States than in continental Europe, where poor public relations of psychiatry in some countries and a greater concern with the possibility of post-traumatic impairment of brain function made psychiatrists unduly reluctant to use frontal lobe surgery. In the United States the number of operations had been increasing steadily, and the arrears of suitable cases in our mental hospitals had not yet been exhausted, when the new tranquillizing drugs appeared on the scene. At this point, largely owing to the advertising slogan that the drugs make surgical brain-destruction a thing of the past, operations were radically discontinued. It is only lately that figures have been slowly rising again, and that patients for whom lobotomies had been recommended but postponed two or three years ago are being reconsidered for the operation, and I must add in some of my own cases of my own observation too late, because in the meantime the deterioration in some has progressed to a point where frontal lobe surgery can no longer be recommended.

If we try to establish the present state of our knowledge, we must clearly differentiate between the two main groups of patients in whom psychosurgery is indicated. The first group deals with chronic schizophrenics in whom all other treatments have been tried unsuccessfully. Aggressive behaviour on one hand and suffering under the threat of delusions and hallucinations on the other hand are the main indications in this group of chronic schizophrenics. The decisive factor for or against the operation in such cases is the degree of deterioration that is present at the time the decision has to be made. It is more and more being recognized that severe deterioration, which usually makes the relatives most desirous to have an operation performed as a last resort, is a serious handicap to a favourable outcome. While it had always been regarded as a pre-requisite that shock treatments be applied properly and extensively before an operation was considered, today it must be requested that pharmacotherapy be given a fair chance in such chronic schizophrenics who have not responded to shock treatments. Ample experimentation with several of the drugs available is in order before psychosurgery should be considered. Careful psychopathological observations are necessary in each case to evaluate the effect of drug therapy. It is not sufficient to have a disturbed patient calmed down to a point where he merely sits around in complete lassitude to the satisfaction of the nursing personnel. It has been evident to observing psychiatrists that chronic schizophrenics whose deterioration has not progressed too far can enjoy life and are able to make a fair adjustment outside hospital

after psychosurgical intervention, while similar patients under tranquillizing drugs are often completely inactive, listless and uncomfortable. In such cases psychosurgery is preferable. It should also be investigated by trial visits home whether or not a patient who responds favourably to the drugs is willing to take the medication once he is outside the hospital. There are also those cases in which the effect of the drugs fades off, and finally those in whom the psychiatrist experienced in psychosurgery evaluation as well as in drug therapy convinces himself that the result obtained with any of the available drugs does not reach the degree of improvement which he would have expected with psychosurgery in this patient. In all these instances surgery should be considered. It will be difficult to demonstrate results obtained in the comparable groups treated with pharmacotherapy and psychosurgery, although research in this direction would be most desirable. Careful psychopathological observation of individual cases is most important, and tends to show clearly that psychosurgery still has definite indications in well-selected cases of chronic schizophrenia.

A group of patients in whom psychosurgery continues to be a most valuable procedure are the cases classified by Hoch and Polatin as pseudo-neurotic schizophrenia. This is equally true for severe and incapacitating obsessive-compulsive neuroses, and it may be permitted to discuss these two groups together as far as the indications for psychosurgery are concerned. It has been shown convincingly by Hoch and his co-workers that these patients, whom we see with particular frequency at the New York State Psychiatric Institute, present remarkable therapeutic results after psychosurgery. Patients who had never been able to lead any normal life, undergo professional training or hold any kind of job, are able to do so for the first time. The figure of 80 per cent. of improvements given for this group goes far beyond anything that could be obtained in such patients prior to the advent of psychosurgery. Since we are often dealing with highly intelligent people, the decision to impair brain function by psychosurgery has always been a difficult one. Therefore, it was obvious that the new drugs should be amply tried in this type of patients and that psychosurgery should be delayed. Although anxiety is their main symptom, pharmacotherapy must be given preference in all cases that are able to lead a satisfactory life under prolonged medication. The term "chemical leucotomy" has been used, but it is not quite correct for various reasons. One major difference is that patients under medication, if they improve, are aware of their previous suffering and feel happy about the relief they have obtained, while the lobotomized patient does not even remember his previous symptoms, at least not spontaneously. If asked about them it may occur to him that he had those symptoms, but he has never thought about them any more. While this could be considered a pleasant state of affairs, it is actually a sign of impairment of critical faculties, and this is not limited to the illness but also concerns other matters. Therefore, even in patients in whom the operation is successful and not too much brain tissue destroyed, the intellectual impairment, regardless of what some of the psychological tests show, is more pronounced than under drug therapy. This is an important factor in favour of the drugs, but it is not a sufficient reason to do away with a valuable operation as long as we do not have better drugs, with fewer side-effects and more therapeutic efficacy.

Psychosurgery became less objectionable when less extensive operations were introduced. The standard lobotomy of Freeman and Watts was given up in favour of a variety of smaller operations leading to fewer personality changes. It was at that time that Freeman himself turned entirely to Fiamberti's trans-

orbital lobotomies as the least extensive operation. In our own experience this method is not sufficient. It is true that psychopathological side-effects are rare, although, as in all blind operations, haemorrhage may occur. The greater safety of open operations in the hands of an experienced neurosurgeon made it easy for us to decide on somewhat larger procedures, and Ranzerhoff, the surgeon operating at the New York Psychiatric Institute, uses pre-coronal operations pretty much of the same type as McKissock here with his rostral leucotomy. It appears, however, that in a number of patients this operation also is not sufficient. I personally feel that re-operations are indicated in cases in whom neither improvement nor side-effects are noticeable after the first operation. It also became apparent that the larger standard operation, with its risk of personality changes, is still indicated in a number of cases, and at the height of our psychosurgical activities, shortly before the advent of pharmacotherapy, the pendulum was again swinging toward the use of more extensive cuts.

Operations using cuts in special cortical areas did not prove to be superior, and are followed by a greater incidence of post-operative epilepsy. Most workers seem to agree that the quantitative principle is applicable to the results of psychosurgery, namely that the effect both on the removal of psychiatric symptoms and on the occurrence of personality changes depends on the quantity of the brain tissue destroyed. A good point in favour of this is that results quite comparable to our own with precoronal operations were obtained by the Boston group with bi-medial operations, and by some others with still different cuts. Attempts to pre-determine the extent of the cut with injections of novocaine prior to the operation, as well as technical modifications using chemicals, electro-coagulation, or ultrasound for the destruction of brain tissue in psychosurgery are still in the experimental stage, and probably they will have to be taken up again more extensively after the premature statement that psychosurgery is a thing of the past has been proved to be unjustified.

It is obvious from the above that the indications for shock treatments and surgery have been greatly modified in the large group of chronic schizophrenics, including the pseudoneurotic type of schizophrenia. This is not quite so in the acute group. The indications for shock treatment appear to have changed little in acute schizophrenia. Attempts with pharmacotherapy are perfectly in order, but they should not be extended beyond the time when shock treatments are no longer of help. It is easy to cover up excitement, delusions, hallucinations and other symptoms of acute schizophrenics with drug therapy. However, they usually recur when pharmacotherapy is discontinued. Since, according to most statistics, shock treatment becomes rather ineffective after six or twelve months of illness, its application should not be delayed for more than a few months, while an attempt with pharmacotherapy is being made. On the other hand, symptomatic psychoses or exogenous reaction types usually clear up under pharmacotherapy alone without shock treatment. Nothing has been changed regarding the treatment of depressions. It is recognized by most workers that all types of depressions continue to be the domain of E.C.T. States of excitement respond to pharmacotherapy in many, but in my experience by no means all instances. Psychotic episodes in some organic brain conditions need no longer be subjected to shock therapy, nor do the withdrawal symptoms of addicts, which respond well to a few convulsive treatments, but better to chlorpromazine or reserpine. I may add, however, that deliria after withdrawal of barbiturates do not seem to be prevented by the newer drugs.

Two points remain to be discussed: the relation between somatic and psychological treatments, and some questions of theory.

The somatic treatments in psychiatry have often been described as an adjunct to psychotherapy, or else they have been rejected altogether because they interfere with the basic concept of some psychiatric schools which claim that the patient himself has to work out his problems. Even some ethical doubts have been raised, as, for instance, that shock treatments change a conscious person with ethical thinking into a temporarily debilitated, primitive, instinct-driven person. In psychosurgery the impairment of ethical and religious feeling had been deplored and used as an objection to this treatment, a point which interfered with the wider use of lobotomies in many European countries. In the United States the interference, temporary or permanent, of the interpersonal relations between doctor and patient has been a frequent objection to any somatic treatment. This objection found a strong resonance in lay publications and was used again and again in attempts to discredit the physical treatments.

In the U.S. the bias against somatic treatments has changed with the introduction of pharmacotherapy, but even their publicity did not do away entirely with the strict alternative: somatic or psychological treatment. There are still some private hospitals which refuse to apply any somatic treatments or limit their use to extreme cases in order to render the patient accessible to the true treatment, which for them can only be psychotherapy, in neurotics and psychotics alike. However, not only the public hospitals but also the vast majority of private hospitals with their large turnover lean heavily on the use of somatic treatments. It is a strange paradox that those hospitals that apply shock treatments may still give lip service to psychotherapy, more so than some European centres for shock treatments. The emphasis which M. Müller in Switzerland and more recently, Hoff in Vienna place on psychotherapy during insulin coma treatment does not seem to have any parallel in insulin units of our hospitals. Even group psychotherapy, used in our public hospitals in spite of the shortage of psychiatrists, is applied mostly to patients not undergoing shock therapy, thus again conforming to the alternative—either somatic or psychotherapeutic approach. The good results of shock treatments in some of our large institutions without any attempt at psychotherapy, in my opinion, is the best proof that the effect of shock treatments does not depend on the simultaneous use of psychotherapy.

This does not mean that psychotherapy of some kind is not desirable in the rehabilitation of patients who have undergone shock therapy with a more or less satisfactory result. This is true for many physical illnesses as well, and it is of course, more so in patients whose sickness has expressed itself in emotional symptoms and in their interference with the patient's interpersonal relations. The patient himself must be taught to understand his previous emotional difficulties, and he and his family must learn to handle their relationship, which has often been changed by residual symptoms of the illness. The various treatments present entirely different problems as far as psychotherapy is concerned. In insulin treatment the transference situation is a favourable one if the same physician watches the patient during his temporary physical and psychopathological manifestations in the state of hypoglycaemia, and is with him during the stage of recovery from hypoglycaemic symptoms, when many patients show a definite need for communicating with the physician. Deep psychotherapy should be left to the end of insulin treatment and limited to individual cases in whom personality changes not subject to insulin remissions remain after the removal of the more dramatic symptoms. In electro-convulsive therapy psychotherapy is hardly possible because of the patient's confusion,

and it is definitely unnecessary in depressions which clear up after a few treatments. Attempts to use the blurring effect of a few electric shock treatments in neurotics under psychotherapy have been made, but are quite unsatisfactory. Neuroses, apart from depressions that occur in neurotic patients, are no indication for electroshock, and its indiscriminate use in such patients with their tendency to conversion symptoms has contributed much to the grudges against the treatment.

A most interesting problem for psychotherapy is presented by psychosurgery, although orthodox psychotherapeutic procedures would be bound to fail here. Active rehabilitation of schizophrenics, which has received much attention by some hospitals, is not an indispensable requirement. Patients become rehabilitated by time, and little difference can be seen between results in public hospitals with little or no post-operative psychotherapy and those in the highly developed post-lobotomy units of private institutions. There is, however, a large group of patients, and this applies particularly to obsessive-compulsive neurotics and pseudoneurotic schizophrenics, in whom only the impact of the symptoms on the patient is reduced, and in whom a psychotherapeutic working out of their problems is possible for the first time after the operation. These patients can be quite successfully taught to live with their symptoms; others will benefit from working with a psychotherapist who is experienced with this type of patient, and as Cattell pointed out, is willing to adjust his technique to the special requirements of these patients.

It is highly disappointing that all theories of the shock treatments were postulated only to be easily disproved. While insulin coma treatment was found incidentally and a theory presented only as an afterthought, convulsive treatment owes its existence at least partly to a claimed antagonism between epilepsy and schizophrenia. This antagonism is neither proven, nor would it explain the fact that affective disorders rather than schizophrenias respond best to convulsive therapy. Both insulin and convulsive treatments have in common that severe pathophysiological changes are brought about by them, as evidenced by numerous neurological manifestations. These organic cerebral manifestations of all physical treatments in psychiatry including psychosurgery are the strongest argument in favour of a somatic cerebral basis of the diseases in which they are effective. The nature of the somatic cerebral changes which influence the disease remain entirely unknown. In E.C.T. electroencephalographic and psychological changes demonstrate a severe disorganization of brain function, but contrary to some recent attempts to correlate the intensity of these changes and the therapeutic effect, it is undeniable that the best results can be obtained with a few convulsive treatments in depressions without such manifestations. Cause and effect are not only mixed up by those who see the appearance of psychopathological manifestations such as amnesia as proof for a psychological theory of the treatment, but equally by those who consider signs of disturbance of cortical function as the organic cause for improvement.

The question whether our treatments are specific or only symptomatic seems highly academic. In my opinion there is definitely something specific in the way shock treatments clear up in a predictable manner at least some psychotic syndromes, even though future episodes of the same illness cannot be prevented. There are few treatments in medicine which are strictly specific for one disease. It is strange that in psychiatry, where until 25 years ago we had no treatments at all, we are stricter in our criteria for treatments than specialists in any other field of medicine. If a symptomatic improvement can be maintained for any length of time it is just as useful as the discovery of insulin for diabetes. The

effect of electric shock therapy in most manic-depressives, who are able to live a normal life for years between their episodes, goes even beyond the effect of insulin for diabetes. I am making this statement because a pseudo-scientific scepticism carried into psychiatry not by clinicians, but by basic scientists and statisticians paralyses therapeutic efforts in our field.

It cannot be denied that statistical evaluation of therapeutic results with any of the treatments discussed here, leaves much to be desired. There are few comparisons available between groups treated in different ways, although our large institutions would lend themselves extremely well to such statistical evaluation under the guidance of experienced clinicians. What is available in statistics is far more contradictory than the experience of different clinicians working under the most varying conditions and arriving more often than not at the same conclusions. The difficulty of statistical proof cannot be a reason to withhold treatments which have therapeutic value even though the degree of their therapeutic effect is still debated. It is also unjustified to discredit treatments by reports that a newer treatment procedure is superior because it achieves results in some individual patients who have failed under previous ones. All treatments should be applied in different stages of the disease if we want to obtain some results. The advent of newer methods should not interfere with the application of previous treatments.

All the somatic treatments, particularly the newer drugs, have contributed to a different attitude on the part of physicians but also of the public toward the psychoses. In the United States, relatives of mental patients are asking us more and more whether maybe it is not so much a childhood experience but rather something chemical in the brain that makes the patient psychotic. Actually, the drugs which brought about this change were also found empirically rather than on the basis of any chemical knowledge regarding the diseases which we are treating. Therefore, there is no reason to be ashamed of the other physical treatments which also have an empirical basis. Neither our knowledge of psychiatric illness nor our treatments stand on firm ground for the time being. I should like to close, however, with an appeal to clearly separate our scientific scruples from our clinical efforts in the individual patient.

DISCUSSION

By **Dr. L. C. Cook**

When I was preparing a critical review of physical treatments for the first volume of *Recent Progress in Psychiatry*, published in 1944, I was always delighted to come upon any papers by Dr. Kalinowsky, because they seemed to me so logical and well-balanced, and I found myself in agreement with nearly everything he wrote. In fact, I felt he must be an exceptionally intelligent and understanding psychiatrist to have come to much the same conclusions as I had myself! So it is with particular pleasure that I follow him this morning, and once again I am not disappointed. I am sure we have all listened with enjoyment and admiration to such a sound and authoritative review of the present position of physical treatments in psychiatry.

Dr. Kalinowsky rightly reminds us that most of the hospitals now in the forefront of modern rehabilitation projects were also pioneers in adopting physical treatments, and undoubtedly physical treatments, by rendering patients more accessible to re-socialization and better able to appreciate normal social activities, have made it much easier to carry out these projects; nevertheless,

he may have missed the point that those men who had the foresight and judgment to take full advantage of the possibilities of physical methods of treatment at their outset were also likely to have the talents and temperament necessary to introduce and steer successfully through storms of criticism and forebodings of disaster the drastic changes in policy involved in giving so much freedom and responsibility to patients who were then considered to be irresponsible and potentially dangerous. I refer particularly to such men as Dr. T. P. Rees of Warlingham Park Hospital.

I am glad that Dr. Kalinowsky does not think that insulin coma treatment should be jettisoned. I have certainly found it to produce a better qualitative remission than E.C.T. in some types of schizophrenia, particularly where ideas of reference and passivity feelings are present without marked withdrawal from reality or superficiality of affect. I prefer to try E.C.T. first in recent florid schizophrenic reactions and in catatonic states, but in recent paranoid reactions of the type just mentioned, I still prescribe insulin coma treatment, with Dr. Kalinowsky's emphasis on adequate depth and number (40-50) of comas. Whether certain tranquillizers alone or combined with convulsion treatment will eventually prove to have an equally good effect, it is, I think, too early to say.

Dr. Kalinowsky speaks of "the slight superiority of metrazol" over E.C.T. in the treatment of some acute schizophrenic states, and I must say that my experience amply confirms this. In particular I have found that the remissions obtained after metrazol are more enduring and of better quality than is generally thought. Four years ago I was able to undertake a follow-up of the 138 female patients suffering from schizophrenia of all types and durations whom I treated with metrazol convulsions between June, 1937 and March, 1940. Of the 56 discharged as recovered or remitted within six months after treatment, we managed to trace all but nine, which, considering the war-time upheaval and destruction in the south-east areas of London where most of them lived, is no mean feat. Of the 47 traced, one was killed in an air-raid three years after recovery, working as an air-raid warden, 30 had remained well with no sign of psychotic relapse, 16 had suffered relapses and 10 of these recovered from their relapse, but several had subsequent relapses. Of the 16 who relapsed, 2 kept well for approximately 13 years before relapsing, 1 for 10 years, 2 for 9 years, 5 for 2½ to 5½ years, and the remaining 6 for 1 to 2½ years. Of the 9 patients we were unable to trace, one was still well 5 years and 2 between 2½ and 3 years after discharge.

I think it is not unduly optimistic to expect that the great majority of the untraced patients have kept out of hospital, because it is the general custom of psychiatric hospitals in this country to write for information if a newly admitted patient has previously been in a mental hospital.

I am with Dr. Kalinowsky in thinking that muscle relaxants do not diminish the strain on the heart. Where serious cardiac risk exists I should feel happier in giving unmodified cardiazol (which, of course, is a heart stimulant), than using any modified procedure. Our practice with E.C.T. at Bexley Hospital is to give brevidil E to prevent fractures and other injuries, but only in a dosage (actually 75 mg. in 1 c.cm.) sufficient to modify, but by no means abolish convulsive movements, and just enough pentothal (0.17 gm. in 3 c.cm.) or evipan (0.33 gm.) to allay anxiety and the unpleasantness of the relaxant. Our long experience with this method has shown that fatalities should not occur, and that the only at all common contra-indication is severe myocardial damage.

The question of having an anaesthetist tempts me to quote another

distinguished American neuropsychiatrist, Dr. Walter Freeman, in quite another context. Some years ago, when his transorbital leucotomy was the subject of rather scathing attacks in some quarters, he told me that he had really introduced this technique because, in what he termed the "backwoods" mental hospitals in America, it was impossible to get a surgeon, let alone a neurosurgeon, to do the many operations necessary. This applies no less to getting anaesthetists in this country for perhaps 4 or 5 daily sessions in a large mental hospital, apart from the not infrequent emergency treatments which should be given forthwith. And in any case I don't think with our technique an anaesthetist is necessary, except perhaps for the peace of mind of some nervous psychiatrists.

Passing to psychosurgery, it is usually considered that a standard leucotomy, i.e. bilateral incisions as far back as possible, is the only method likely to be successful in schizophrenia. If that is so, surely few would disagree that all other treatments with less danger to life and to the personality should be tried first; and as the results of the phenothiazine tranquillizers and of reserpine have in many cases been so successful, I do not feel justified in advising standard leucotomy until these tranquillizers have been proved ineffective. After all, it only means six months' delay. The difficulty comes when the more severe symptoms are eliminated by tranquillizers, leaving a schizophrenic residue, which may or may not be remedied by leucotomy. In considering the problem presented by these cases it must be remembered, as Dr. Kalinowsky points out, that standard leucotomy tends to impair the critical faculties, while pharmacological treatment does not.

It is heart-warming to find an American psychiatrist so courageous as to say categorically "the effect of shock treatment does not depend on the simultaneous use of psychotherapy"; I fully agree that the physical treatments can and do produce complete recoveries from psychotic illnesses without psychotherapy. Nevertheless, there is little doubt that most psychotic breakdowns have both physiogenic and psychogenic antecedents, while the illness itself often produces social and domestic problems. It would amount to gross neglect not to explore and try and solve conscious and unconscious psychological difficulties, as well as the practical problems so likely to beset a patient remitting from a psychotic illness, and I am sure all psychiatrists, however materially minded, try to do this.

Dr. Kalinowsky's warning against giving E.C.T. in most neurotic conditions is necessary and welcome. It is sometimes particularly tempting to give E.C.T. to hysterical hypochondriacs who claim to be miserably depressed, but these are the very patients most likely to annexe the temporary side-effects of the treatment, such as headaches, nausea, memory defect, etc., as a permanent addition to their complaints, as a heaven-sent cause for grievance and as a plausible reason for their woeful state of invalidism. On the other hand, syndromes presenting as purely reactive depression sometimes clear up remarkably well with a few E.C.T.s.

I am interested in Dr. Kalinowsky's observation that patients subjected to psychosurgery have been found to fare equally well with or without rehabilitation psychotherapy. I have found that, after standard leucotomy especially, tactful but firm stimulation is often essential for longish periods before discharging the patient, and that if he is sent home to unsympathetic or impatient relatives before being fully rehabilitated, disaster is likely to follow.

It should not be necessary to have to defend the use of physical treatments

in psychiatry after 20 years of successful usage, and I think there are very few psychiatrists in this country so bigoted as to say that none of them should ever be used. At the same time there exists strong divergence of opinion as to the scope of their usefulness in general, and especially concerning the choice of treatment in various conditions and the advantages and disadvantages of different techniques; and although most of Dr. Kalinowsky's opinions happen to coincide with my own, I feel sure that many in the audience will have dissenting views. For this reason I am rather sorry that the discussion is limited to questions, but I have no doubt that questioners will be ingenious enough to adopt the well-known parliamentary "Supplementary Question" technique in order to make clear their real views.

THE USE OF TRANQUILLIZERS IN PSYCHIATRY

By

PAUL H. HOCH, M.D.

*Commissioner of Mental Hygiene, State of New York
Professor of Clinical Psychiatry, Columbia University*

I SHOULD like to discuss some of the clinical aspects of the tranquillizing drugs, but without going into great detail as to the many theoretical considerations in biochemistry, physiology, pharmacology, etc. which have been ventilated so often in connection with this form of treatment.

In the last few years more and more chemical compounds have been introduced that influence mental states. The older group of drugs or sedatives were used usually to reduce emotional tension, to relieve anxiety, to induce sleep, and to eliminate excitement. Up to a point these compounds influenced mental symptomatology, but they were not very effective. The recently introduced tranquillizing drugs also have, as we will discuss later, a sedative action; but they are far more effective in the treatment of mental illness than the sedatives that formerly were used.

The number of such drugs has been growing rapidly. At this time I cannot mention all the different compounds which are now in use, and will limit myself to chlorpromazine and the Rauwolfia preparations. Many of the new compounds are variations of the above-mentioned drugs. The action radii of chlorpromazine and the Rauwolfia compounds are fairly well known today and could be used as a comparative baseline in evaluating the newer compounds. The selection of drugs to be used for treatment is still made largely on the basis of the therapist's preference rather than on particular indications. In many instances the actions of these drugs overlap. However, special considerations have to be followed in their use. For instance, in acute mental disorders where there is an excitement or a state of agitation we prefer the use of chlorpromazine because it is a quick-acting drug and therefore more effective in this particular group of patients than Rauwolfia. We feel that in the treatment of chronic patients, too, chlorpromazine is superior to Rauwolfia, but it must be mentioned that chlorpromazine produces more side reactions. This should be taken into account in some clinical situations—for instance in a patient suffering from a known liver ailment or gall-bladder disease we would prefer treatment with one of the Rauwolfia preparations.

Clinical experience shows that individual patients respond better to one drug than to another, but we have no evidence as to why this is so. Neither the psychiatric state of the patient nor the known biochemical facts regarding the action of these drugs give sufficient clues to enable us to decide why a patient responds better to one compound than to another. Statistics compiled in the State of New York and in many other places indicate that chlorpromazine is probably the most effective drug in our hands in the treatment of psychotic patients. This is followed by reserpine. Other available compounds such as Frenquel and Miltown are far less effective. Sparine (promazine) is effective though apparently milder in action than chlorpromazine, but also has fewer side effects.

Most of the information we have on the action of tranquillizing drugs relates to psychotic patients. This is not surprising, because patients with gross symptomatology are better subjects to be studied in determining the validity of drug action than the milder disorders which do not have as conspicuous a symptomatology. The investigative techniques—for instance, the double-blind method—also can be more effectively applied in a hospital setting than in a clinical or private practice. The evaluation of hospitalized patients is therefore much easier than that of patient treated in an ambulatory fashion.

Indications for the use of the tranquillizing drugs are not sharply drawn. From some publications one might assume that these drugs can be used in every condition regardless of origin, and that every psychiatric patient provided he takes the drug long enough and in sufficient doses can be cured. Unfortunately, this is not the case. Even though many patients benefit from the use of these drugs there is still a very large number of patients who do not respond. This is especially true regarding patients who have been sick for a long period of time.

All investigators agree that these drugs are most effective in psychotic patients who are excited, disturbed, tense, or anxious, and who have productive psychotic symptoms. Apathetic, driveless, non-productive patients do not respond well to tranquillizing drugs. There are exceptions, however, which occur in those patients where behind an apathetic front there is a great deal of hidden tension. As far as we know the drugs do not influence a disease or disorder; they eliminate or alleviate symptoms only; nevertheless in a large number of patients the elimination of the most disturbing and conspicuous symptoms is sufficient to enable the patient to function.

Although the drugs may be used on a large group of patients simultaneously, this does not mean that the patients should not have an individual psychiatric and mental examination and that the psychiatric symptoms should not be properly appraised. If this rule is not followed, many patients will not benefit who otherwise may have responded well and complications will be overlooked. It is also essential to pay attention to what other psychiatric attention or treatment the patients needs in addition to the drug treatment.

Referring to the special psychiatric disorders I would like first to discuss schizophrenia, in which these new compounds are most effectively applied. They are used here in three different ways, in the treatment of acute hospitalized schizophrenics, chronic hospitalized schizophrenics, and schizophrenic out-patients.

Today the use of tranquillizing drugs in acute schizophrenia is widespread in the in-patient services, but less is known about their value in the out-patient services or the offices of physicians in private practice. Statistics available today indicate a drop in the admission of schizophrenic patients. In our experience there are some acute cases of schizophrenia that respond well to tranquillizing drugs and it has been possible to treat them outside hospital. However, this is only possible when proper supervision is organized in co-operation with the relatives. Where such supervision cannot be obtained and where the environment is not willing or able to co-operate, ambulatory treatment is not possible. We also feel that depressed and anti-social patients should be treated in hospital. Generally speaking, in cases where the higher dosages of tranquillizing drugs have to be used the patients are best treated in hospital, but once the maintenance dose is established these patients can be returned to ambulatory treatment. We feel that the main contribution of the drugs at present is the shortening of the patient's stay in hospital.

The tranquillizing drugs are widely advocated in the treatment of chronic schizophrenic patients. Undeniably the excellent results can be maintained for many years in cases that have not responded to other forms of treatment. Our observations on a large number of patients would indicate that about 10 to 15 per cent. of the patients improve to such a degree that they are able to return to the community. Many other patients show improvement in behaviour and are easier to care for, but must still remain in a hospital. In many chronic schizophrenic patients it is possible to influence the gross symptomatology and especially to reduce and eliminate the hallucinations and other psychotic manifestations. Many of these patients, however, still show adaptational difficulties in work, social, and sexual spheres. The tranquillizing drugs do not influence uniformly all the symptoms of schizophrenia. Symptoms such as anxiety and aggression are influenced most effectively and reliably; disorganization of thinking is less influenced.

The different forms of schizophrenia all respond to the tranquillizing drugs, but to varying degrees. The paranoid and catatonic forms respond best and the hebephrenic and simple forms less well. The tranquillizing drugs are also used in depressed types of schizophrenia, where they have some influence on the psychosis itself, but generally not on the depressive manifestations. In such patients a combination of tranquillizers with an anti-depressant should be used. These drug combinations, however, are not ideal, and we still feel that electroshock is the best treatment. In the pseudo-neurotic form where a great deal of anxiety is present the tranquillizing drugs are not generally able to reduce the symptoms.

The tranquillizing drugs are used in the treatment of the manic-depressive psychoses. Some feel that the phenothiazine derivatives can be used effectively in cutting short severe manic states if the drug is given by injection. Later on when the patient quiets down he is transferred to oral medication. In milder manic attacks the treatment can be omitted. Manic patients quieten down fairly rapidly under the influence of chlorpromazine, but there are exceptions which do not respond, or show a recurrence of manic manifestations when the drug is reduced or withdrawn.

The drugs are not effective in depressions. This applies to patients suffering from reactive as well as neurotic depressions. The only exceptions are agitated depressions, where the drug may cut short the agitation even though the depression remains uninfluenced. It must be emphasized that the tranquillizing drugs not only do not help depressed patients, but can aggravate existing depressions and even precipitate depressions in those formerly free.

It is not known how far the preventive use of tranquillizing drugs between attacks will prevent recurrence.

Tranquillizing drugs are used in arteriosclerosis and senile psychoses where they have a beneficial effect on the patients' behaviour and in controlling delusions and hallucinations. These drugs calm the patient without interfering too much with the patient's behaviour. Many of these patients formerly had to be sedated, but the sedatives even though calming the patient interfered with consciousness. The newer tranquillizing drugs do not have this effect, and it is commonly seen that the patient's confusional state instead of being aggravated is improved. The drugs, of course, have no influence on the intellectual impairment of these organic states, but they have a strong influence on the mental processes most likely to occur, because some emotional interference is removed. The tranquillizing drugs are also used in acute organic states such as delirium and have a beneficial effect if hyperactivity and agitation are present. Here

again their use is preferable to that of sedative drugs, because they do not interfere with consciousness, but rather reduce than reinforce confusional symptoms.

In spite of the widespread use of the drugs in the United States, our knowledge about their effectiveness, however, is not too well documented in their use with psychoneurotic patients. Some such patients with tension, phobic manifestations respond to the drugs; others with a similar symptomatology do not. Why one patient responds and the other is refractory is unknown and we have no biochemical clues as to how this occurs. Some neurotic patients undoubtedly benefit from these drugs, and in our experience it does not reduce or limit their ability to participate in psychotherapy.

I do not want to go into a lengthy discussion of dosages, but should like to call attention to the fact that the individual dosages must be worked out. There are patients with very similar symptomatology who need for instance 100 mg. whereas, other patients need 1,000 mg. or more. In the United States there is a more or less uniform procedure to increase the dosage to the extent needed to eliminate the patient's symptoms. Some physicians increase the dosage rate rapidly, while others follow a slower course. There are adherents to techniques that prefer large doses, whereas others feel that they can get along with relatively less. We feel that a generalization of small or high dosages should be used with the recognition of the fact that individual patients respond differently to the drug and the drug has to be adapted to the particular patient's need. The usual technique is to raise the level to control the patient's symptoms, leave the patient on that dosage level, and then gradually reduce the drug or eliminate it altogether if the patient shows a gratifying response. We usually start out with 25 mg. three times a day and then work it up, if necessary, to 1,500 mg. daily. There are therapists who have used much higher dosages than this. In reserpine we usually administer between 3 and 5 mg. These dosages do not apply automatically to all available drugs.

The duration of treatment is often a matter under discussion. We feel that a patient should be treated for several months and preferably with two or three compounds if he does not respond. If a patient does not respond within a few weeks to one compound, it is advisable to change. Some patients for instance respond better to one drug than to another. But there are patients who do not respond satisfactorily to any of the available drugs. The maintenance dosage has to be individualized. Patients who receive maintenance drug treatment should be seen frequently by their psychiatrists or physicians to have this dosage adjusted. Many patients will need a long-range treatment, and some will have to be kept on a maintenance dosage for many months and sometimes longer or the symptoms will re-appear. Once the patient is comfortably established, the medication should be reduced or eliminated altogether. The tranquillizing drugs are used either alone or in conjunction with other psychiatric therapies. Some patients seemingly have enough ego strength and are able to function well if the drug eliminates the most disturbing features of the emotional disorder. Others need other psychiatric therapy in addition. This can be in the form of milieu therapy, group therapy, or individual therapy. Many patients under the influence of the drug become more co-operative and are more ready and willing to discuss their adaptational problems. This, of course, would indicate that they would like to have a therapist with whom they can discuss some of their problems. Some therapists feel if the patient's symptoms are reduced or completely eliminated by the drug they have no incentive to undergo psychotherapy. This is especially noted in those patients suffering from the

psychoneuroses. We have not had such an experience and feel that patients who have conflicts will seek psychiatric help to resolve them.

I cannot discuss at length here the complications produced by the tranquillizing drugs. Fortunately they are not very serious and usually can be controlled by withdrawal of the drug. I should like to call attention here to the fact that in addition to physical complications such as jaundice, skin conditions, agranulocytosis and Parkinsonism, these may also be psychic complications which should be watched for. In some patients the drugs can produce peculiar feelings of depersonalization, depression, and lethargy. Occasionally new psychotic manifestations may develop. These complications are not serious and can be dealt with by changing the dosages or by switching the patient from one drug to another.

The mode of action of the tranquillizing drugs is still obscure and most of our treatment knowledge is empirical. There are hints that the reticular substance and mid-brain region are implicated, but details as to their relation to emotional and mental disorders are controversial. Our therapeutic approach should not be looked upon too critically because it is empirical, since progress in medicine has been achieved most impressively on an empirical level. Of course, we all hope that the present investigations in neuropharmacology will elucidate some of the causes of different mental disorders and clarify the action of these drugs.

BIBLIOGRAPHY

1. ANNALS OF THE NEW YORK ACADEMY OF SCIENCES, "Reserpine in the Treatment of Neuropsychiatric, Neurological, and Related Clinical Problems", **61**, Art. 1, pp. 1-280. New York. 15 April, 1955.
2. CHLORPROMAZINE AND MENTAL HEALTH. Proceedings of the Symposium held under the auspices of Smith, Kline, and French Laboratories, 6 June, 1955. Warwick Hotel, Philadelphia, Pa., 1955. Philadelphia: Lea and Febiger.
3. DELAY, J., and DENIKER, P., "38 Cases of Psychoses under Prolonged and Continuous R.P.4560 Treatment", *Compte rendu du Congrès des Méd. Aliénistes et Neurologistes*, Luxembourg, 21-27 July, 1952. pp. 7-18.
4. *Idem*, "Use in Psychiatric Therapy of a Phenothiazine with Selective Central Action (R.P.4560)", *Ann. Méd.-Psychol.*, 1952, **110**, 112.
5. ELKES, J., and ELKES, C., "Effect of Chlorpromazine on the Behaviour of Chronically Overactive Psychotic Patients", *Brit. Med. J.*, 1954, *ii*, 560.
6. HOCH, P. H., MALITZ, S., and LESSE, S., "A Two-Year Evaluation of Chlorpromazine in Clinical Research and Practice", *Amer. J. Psychiat.*, 1956, **113**, 540.
7. *Idem*, "New Drug Therapy in Psychiatry: Clinical Uses and Abuses in Mental Disorders", *N.Y. Acad. Med. Bull.*, 1957, **33**, 474.
8. *Idem*, "The Production and Alleviation of Mental Abnormalities by Drugs", XXth International Congress, Brussels, Belgium. 1956. St. Catherine's Press.
9. KINROSS-WRIGHT, V., "Chlorpromazine Treatment of Mental Disorders", American Psychiatric Association at 121st Annual Meeting. Amer. Assoc. Adv. Sci., Berkeley, Calif., 1954.
10. LEHMANN, H. E., "Therapeutic Results with Chlorpromazine (Largactil) in Psychiatric Conditions", *Canad. M.A.J.*, 1955, **72**, 91.
11. WINKELMANN, N. W., JR., "Chlorpromazine in the Treatment of Neuropsychiatric Disorders", *J. Amer. Med. Ass.*, 1954, **155**, 18.

DISCUSSION

By Dr. M. A. Partridge

It is always a pleasure to hear Dr. Hoch, and now that he is a Sultan of the Psychoses with 125,000 patients under his clinical tutelage, it is more than ever a privilege to have the opportunity of learning from his experience. I am not the best person to open a discussion of his paper, because I have in my practice neither the opportunity nor patients of suitable type for the sort of extensive

trials that are necessary for the general assessment of tranquillizing substances.

The fashionable complaint today is tension, and the exasperating phrase "I'm all tensed up" is frequently used by patients who show no objective evidence of tension at all, and tranquillizers are even sometimes asked for by patients who do not recognize that somatic anxiety symptoms are sometimes a normal physiological response. There would seem to be various reasons for the increasing trend, as it appears to be, towards seeking freedom from anxiety symptoms. Among these is the fact that we live in an age of technology during which the often spectacular technical achievements encourage the belief, especially among Americans, that to every problem there must be a technical solution. These ideas are not discouraged by the great drug firms whose remedies, breeding like rabbits, insist on their diseases; and they are enhanced by the greater dissemination of medical knowledge, as well as by the activities of journalists who write up their own experiences of drugs, or those of others at secondhand. It is as though behind the frequent applications for help that one receives there lies a wish to avoid the less pleasant aspects of emotion rather than to forego emotion as a whole. This effect is not achieved by tranquillizers, which in my experience are in general unhelpful in neuroses. In this connection mention may be made of the recent experiment conducted at St. George's Hospital in which six substances (four tranquillizers, amytal, and a placebo) were given to a series of neurotic patients over a period of two weeks each, under an ingenious statistical arrangement by which each acted as the others' control. The results were of interest in that the only significant effect on neurotic symptoms was exerted by amytal.

In general, and as regards all conditions, I feel that I could dispense with Benactyzine, Sedalutin, Pacatal, Oblivon, and Anxine, and who now remembers Myanesin (mephenesin) and Seconesin—at any rate in psychiatric practice?

For practical purposes, that leaves us with meprobamate (Equanil or Miltown), reserpine (Serpasil), and chlorpromazine (Largactil). With regard to the first, I have been unimpressed, though my younger colleagues believe that in some instances it is symptomatically useful in patients who have shown an only partial recovery from endogenous depressions with lingering anxiety features. Of reserpine, I have practically no direct knowledge but there seems no doubt that it is of value in mental hospital practice for the reduction of agitation and excitement, while the fact that it is liable to produce attacks of melancholia, usually, it seems, between the fourth and sixth month of its first administration, renders it a substance of great theoretical interest. As to chlorpromazine, again there seems no doubt that in mental hospital practice it is of great value also in allaying agitation and excitement, particularly in senile cases, and particularly in deliria. I myself have found it superior to other methods of treatment in only one condition, namely paranoid schizophrenia, and particularly in the well-preserved type of paraphrenic who tends to be so resistant to other measures. Contrary to the views that Dr. Diethelm expressed yesterday, I have not found it valuable in depressive cases, with one possible exception, but in view of the remarks that have been made at these meetings I may have to revise my views, not for the first time.

If this brief dismissal of the subject seems hasty and ill-considered, it must always be kept in mind that even if the value of tranquillizers is confined mainly to chlorpromazine and reserpine, these nonetheless represent a real advance, and if the value of these is again confined to the treatment of excitement, agitation and paranoid schizophrenia, it is a real gain that they work at all, while the possibility of further contributions to knowledge arising from their

further study is also a real one. Dr. Hoch's indications and figures as to the cancellation, because of the improvement wrought by these substances, of planned mental hospital projects, and of the vastly improved atmosphere that has come about in the mental hospitals under his charge, are so impressive as to speak for themselves.

BIOCHEMISTRY AND SCHIZOPHRENIA

By

HOWARD D. FABING, M.D.

It is presumptuous for a clinician to embark on this subject, and I hasten to apologize for my boldness in doing so. My excuses are many. The first comes from clinical experience. Since my student days, when I observed malignant schizophrenia in a young girl for the first time, and watched its relentless progress despite all therapeutic efforts, I came to the belief that this was an illness of body as well as of mind, and this belief has led me to maintain a continuing interest in the medical, as distinct from the purely psychological, investigations of this disease throughout my professional years. Next has been the observation of experimental or model psychoses produced by drugs. An LSD or mescaline psychosis is not schizophrenia, any more than curare poisoning is myasthenia gravis, but there is enough similarity between the dissociation states produced by psychotomimetic drugs and the real thing to stimulate one's curiosity, at least.

Physical treatment methods of schizophrenia, including insulin and electro-convulsive therapies, as well as the new pharmacological agents, especially the Rauwolfia fractions and the phenothiazines, have made their impression on me as well, as they have on many of you. Their ability to bring about cessation of the schizophrenic process is far from consistent, but enough gratifying results have occurred to catch the fancy of any student of this disease, and to make him wonder about the ways in which they exert their effects. My final excuse for tackling this subject of biochemistry and schizophrenia is the one of good fortune. It has been my privilege to come to know many of the people working in this field in the United States and Canada in recent years, and to listen to them in both formal and informal moments. I come, therefore, as a reporter of work in progress from the hands of some of my colleagues in North America. This is an account of some of the straws they have thrown into the wind, and of the directions in which they seem to be blowing.

The search for a biochemical "lesion" in schizophrenia is a long story and needs no review here. I shall confine myself to more recent investigations. At the present time there is a renewed search for an endotoxin or for multiple endotoxins, produced through metabolic error, as the answer to the riddle of schizophrenia. This hypothesis holds that the patient afflicted with the disease, possibly because of an abnormality in his or her genes, comes into the world with a chemically determined predisposition, and that it may flower into clinical symptoms at varying age levels from one case to another. It is generally held that the basic difficulty is in enzyme systems, and that as the result of such enzymatic fault, chemical substances poisonous to the brain are generated, and that the patient literally stews in his own juice as a result.

If such a derangement of one's juice is present in schizophrenia, it should be possible to transfer body fluids from the healthy to the sick, and to note behavioural changes. This has been done, and with occasional suggestive results. Reiter (1938) performed exchange transfusions on four schizophrenic patients, using blood from non-psychotic subjects as donors. Three of the four showed

improvement, which was impressive in two cases, and it lasted for three to four weeks.

Recently Freedman and Ginsburg (1957) have repeated Reiter's experiment in four cases. The first was one of identical twins, aged $5\frac{1}{2}$ years. They were suffering from childhood schizophrenia. The sicker twin underwent exchange transfusion, while the less sick twin acted as a control. The donor blood came from a non-psychotic adult. It was estimated that 55 per cent. of the child's total blood was exchanged. As the transfusion proceeded, the child's initial restlessness changed to tranquillity. He became more aware of his surroundings in succeeding days, attempted to play simple games, showed evidences of affection towards others he had never exhibited before, and began to babble, although he had never talked previously. The improvement began to recede after the third week, and he reverted to his former severe schizophrenic state in the fifth week. The other twin, living under identical conditions, showed no change during the experimental period. These investigators were unable to obtain benefit in three chronically ill *adult* patients in which this procedure was followed.

What occurs when we look at the other side of the coin, when blood or a blood extract from schizophrenics is introduced into the circulatory system of normal non-psychotic recipients? This brings us to the studies of the Tulane University group under the direction of Dr. Robert G. Heath (1957a). Recently they have made rapid injections of 450 ml. of plasma from schizophrenic patients, to which 50 ml. of citrate has been added, into the veins of non-psychotic volunteer prison inmates after 500 ml. of their own blood had been withdrawn. With the help of a hand pump it was possible to effect this intravenous plasma injection within a space of 2 to $3\frac{3}{8}$ minutes. All of four volunteers receiving plasma from schizophrenics developed symptoms, described as being characteristic of schizophrenia, which persisted for 15 to 45 minutes. The reactions were not intense, but all subjects showed lessening in facial animation, and all described symptoms of depersonalization, blocking and thought deprivation. One showed a mild degree of posturing. Although the reactions were mild and without clear-cut secondary symptoms such as hallucinations, they were all definite. Two subjects receiving plasma from non-psychotic donors showed none of these aberrations. The experiments were conducted under double-blind conditions.

In addition to this experiment of rapid whole plasma injection from schizophrenic patients into non-psychotic normals, the Tulane group has been occupied during the past two years with the study of serum concentrates injected in the same fashion. They have made a protein extract from the blood serum of schizophrenics (Heath *et al.*, 1957b) which they call taraxein.* This extract has been given to 21 human non-psychotic volunteers. The dose administered in each case was that quantity of taraxein which could be derived from 400 ml. of schizophrenic serum. Similar extracts from non-schizophrenic blood were used as controls, and a double-blind technique was used to assess results. All subjects who received the concentrate from patients ill with the disease developed symptoms which have been described for schizophrenia. Fundamental symptoms such as blocking and deprivation of thought, depersonalization and autistic feelings developed consistently, and persisted for two hours or longer. Each of the classical symptoms, such as catatonic stupor and excitement, hebephrenia, ideas of reference, delusions of grandiosity or persecu-

* From the Greek *tarasso*, to throw into turmoil or disturb the mind; similarly the term ataractic is applied to drugs with the opposite effect.

tion, and auditory hallucinations appeared in one or more of the test subjects. No visual hallucinations were reported. Heath informs me that these observations have been duplicated by Swedish investigators recently.

Another recent report on the behavioural effects which result from the injection of schizophrenic blood plasma is worthy of account. In this case the albino rat (Holtzman strain), a less complicated creature from the standpoint of behaviour than his human cousin, was used as the recipient. Winter and Flataker (1951) developed a rope-climbing test for rats in order to compare their responses to various anti-histaminic drugs. The rats were trained to climb a vertical rope approximately five feet high (172 cm.). They were fasted overnight, and a dish of food placed on a platform at the top of the rope served as an inducement for climbing the next morning. As an immediate stimulus to climb the rope, mild electric shocks were applied to the rats' feet through a wire grid on the floor of the cage. After a week to two weeks of such training, these laboratory rats learned to climb the rope in approximately four seconds after the electrical stimulus was applied, and there were only minor variations from rat to rat and from trial to trial.

This technique lends itself to accurate timing within 0.1 second intervals and to the expression of the rats' response in quantitative terms. Climbing-time was measured at intervals after injection of the drug over a 3-hour period.



FIG. 1.

When the climbing time was plotted against the time after injection of a drug, an accurate measure of the degree and the duration of the drug's effects could be obtained in units which they called "minute-seconds".

Winter and Flataker (1956) also found this rope climbing test to be an efficient quantitative method for expressing LSD effects in the rat, and more recently (1957) they have turned to the study of extracts from the blood of humans in health and disease. Heparinized plasma from psychotic patients not on treatment with ataractic drugs (Fabing, 1955) was obtained from three mental hospital populations and from a group of non-psychotic volunteers, part of whom were in good health and part of whom were patients suffering from medical and surgical disorders. Animals were given 1 ml. injections of plasma under blind experimental conditions in which the experimenter was unaware of the diagnosis associated with the plasma samples. Three rats were injected in each test. Seventy-eight psychotic patients and seventy non-psychotic subjects provided plasma specimens for these studies. The following table summarizes their results:

Group	Diagnosis	No. of Patients	Average C.T.D.* min.-sec.
I	Schizophrenia, paranoid	17	1,073
II	Schizophrenia, catatonic	10	720
III	Schizophrenia, childhood	3	800
IV	Schizophrenia, hebephrenic	4	249
V	Schizophrenia, undifferentiated or not specified	11	920
VI	Miscellaneous	8	991
VII	Alcoholic psychosis	3	514
VIII	Mental deficiency with psychosis	4	274
IX	Manic depressive	4	485
X	Schizoid personality	3	155
XI	Emotionally unstable	2	67
XII	Brain trauma	1	164
XIII	Volunteers, normal health	44	147
XIV	Non-psychotic medical and surgical patients ..	26	149

* C.T.D.=Climbing Time Delay.

The delay in climbing times in the rats who were injected with plasma from psychotic patients, and especially from the schizophrenic groups, is demonstrated clearly by the table. The data have been subjected to various kinds of statistical analysis, and it is concluded that the differences are highly significant. In addition to the stark objectivity of these figures, the investigators have described profound behavioural changes in the rats receiving intraperitoneal injections of the more potent plasma specimens from schizophrenic patients. Instead of being frisky and anxious to be handled, the rats retreated to the back of the cage, huddled together and became quiet and withdrawn. Their climbing technique was clumsy and was often interrupted, while the animals seemed to stare into space. If they achieved the platform at the top of the rope, they often hung their heads over the food box without eating. Much of this behaviour appeared reminiscent of the activity of the same rats under intoxication with LSD. Experiments were also carried out with injections made subcutaneously, but effects were lacking. Similarly, intraperitoneal injections of cerebrospinal fluid from schizophrenics failed to produce climbing time delay and changed behaviour.

Let us go back to our original question. Is the schizophrenic patient stewing in his own juice? It seems that the experiments detailed above give a

hint that when much of the blood of at least some schizophrenics is washed out and replaced by blood from non-schizophrenics, there is a temporary amelioration in the psychotic process. The obverse of this type of experiment seems more convincing. The juice of schizophrenics, in the form of blood extracts, appears capable of producing transient schizophrenic symptoms when it is incorporated into the circulation of normal rats and men. This should not surprise us, because it is considered with other findings of biological differences between schizophrenic and normal blood sera, such as the finding almost 30 years ago (Lazell and Prince, 1929) that the serum of schizophrenic patients kills the tadpoles of *Rana catesbiana* in three days, whereas these tadpoles lived in normal serum, plus Macht's (1948, 1950) observation that schizophrenic serum inhibits the germination of the seeds of *Lupinus albus*, Fischer's (1953) observation that it is toxic to larvae of *Xenopus laevis*, and Federoff's (1956) recent observation that it is toxic to the "L" fibroblast derived from mouse derma in tissue culture preparations. At Zurich last week we learned about another of these biological differences. Workers in Gyorgi's laboratory have found schizophrenic serum toxic to mycelia of certain moulds and fungi.

Perhaps none of these experiments alone is totally convincing that a biochemical difference between the blood of schizophrenics and of the remainder of our race exists, but the sum of the evidence is strong enough to have set a great many minds into motion among our colleagues in the field of chemistry. The most revealing creatures seem to be the rats climbing their ropes. Winter plans to continue his observations and to expand his technique into other areas of biochemical inquiry in mental illness. The happiest aspect of the rat climbing test is that it can be studied quantitatively. One of the difficulties which has impeded research in psychiatry is that there has been so little that we could weigh and measure, and all science suffers when these things cannot be done.

If, then, we accept the above data optimistically and say that there is a toxic substance or that there are toxic substances in the blood of schizophrenics, what is its nature or what are their natures? There, of course, is the rub. Nobody has brought the culprit to heel.

It may be worth while, however, to list the major areas where abnormal compounds have been sought for in this quest in recent years:

1. Adrenal cortex (Pincus and Hoagland, 1950).
2. Imidazoles (Young *et al.*, 1951).
3. Adrenal medulla (Funkenstein *et al.*, 1952).
4. Glutathione (Altschule *et al.*, 1952).
5. Adrenochrome (Hoffer *et al.*, 1954).
6. Adrenoxin (Rinkel *et al.*, 1954).
7. Serotonin (Erspamer, 1954; Brodie *et al.*, 1957, *inter alia*).
8. Indoles (Hoffer *et al.*, 1954; Fabing, 1955).
9. 5-hydroxyindoles (Wooley and Shaw, 1954).
10. Aromatic compounds (McGeer *et al.*, 1956).
11. Tryptophane metabolites (Sherwood, Riegelhaupt, 1956).
12. Serum copper (Leach *et al.*, 1956; Akerfeldt, 1957a; Abood, 1957a).
13. Histamine (Lovett Doust *et al.*, 1956).
14. Catechol amines (Abood, 1957b; Sulkowitch and Altschule, 1957).

The list, though not complete, is a formidable one, and represents a prodigious amount of effort on the part of many workers. It is interesting to look down this list and to note, especially during the last three years, the number of investigations which concern themselves with compounds containing

benzene rings. Among them are all the indoles, including the 5-OH indoles such as serotonin, adrenochrome and other catechols, and tryptophane itself, as well as other aromatic compounds. It is noteworthy that the human

AROMATIC GROUPS UNDER STUDY

(with examples of each)

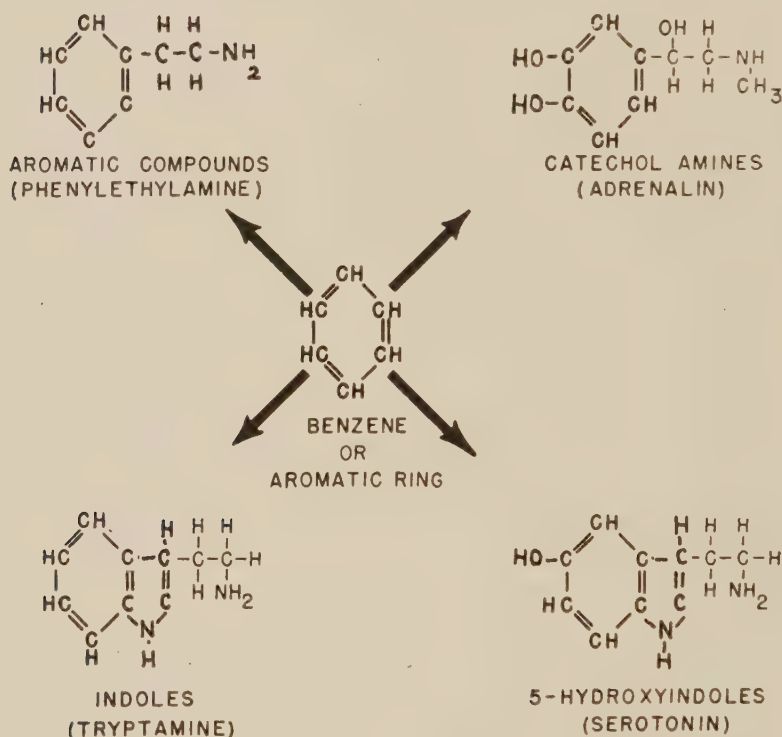


FIG. 2.

body, despite its constant and amazing accomplishments in synthesis, does not seem to be able to make the benzene or aromatic nucleus *de novo*, and that it must be introduced in our food. The usual food sources for this ring structure in our diet are in the amino-acids phenylalanine and tryptophane. We get these in our protein intake.

I am reminded of a remark which Romola Nijinsky made to me twenty years ago. Her whole life was dedicated to the search for a cure for her husband, the renowned Russian dancer, and she became a close student of his illness. She said, "Why do so many schizophrenics quit eating meat? Doctors never take note of this fact, but it is so. Could it be that the patients sense that it is harmful to them and avoid it in an almost instinctual way?" It would seem that if her observation is correct, a good many people are trying to answer her question at the present time.

The great majority of the hallucinogenic or psychotomimetic or psychedelic (Osmond, 1957) drugs which have been undergoing intensive study in recent years contain the benzene nucleus, either as indoles or as aromatic amines. The dramatic episode of Hofmann's (Stoll, 1947) stumbling upon the effect of LSD, a benzene-ring-containing indole, in himself in 1943 has undoubtedly

focused the attention of many investigators on this group of compounds (Fabing, 1956c). The story of his accidental swallowing of a small quantity of lysergic acid diethylamide, inadvertently sucked into his mouth from a pipette, and the consequent temporary schizophrenia-like psychosis it produced, has been told many times. There are two other laboratory accidents of this kind which have never received common notice.

The first concerns W. K. Sherwood (1956), of Monterey, California, a student of tryptophane metabolism. He was working with a peroxidated derivative of tryptophane produced by the mould, *Neurospora crassa*. He assigned the term "B.G.E." to the oily yellow compound which he was extracting, because it produced a blue-green colour when Ehrlich's reagent was added. When the substance was heated, fumes were generated which, when inhaled, produced schizophrenia-like symptoms. Under these circumstances he became intoxicated on repeated occasions. He became withdrawn, uncommunicative, dissociated from the world of reality, and incapable of normal activity. His thought processes became blocked and he sat and stared at his laboratory bench for long periods, involved in a jumbled tangle of thoughts. He experienced depersonalization symptoms in that he felt that an "Outside Talker" was doing his thinking and speaking for him. Auditory hallucinations, in the form of conversations with the Outside Talker, occurred. These disturbed states of thought and behaviour persisted for as much as four to seven days, and he found that he had to get away from his laboratory and its fumes for periods of as long as a fortnight before he recovered fully. His wife came to recognize the onset of his symptoms before he did. She would announce, "You've been smelling things again", and would insist that he stay away from his laboratory until he had recovered.

The second instance of accidental model psychosis occurred in a commercial laboratory which was interested in the electronic sterilization of foods. In the case of meats it was found that they developed an offensive odour of skatole when processed in this manner. A chemist was assigned the task of irradiating tryptophane and otherwise manipulating it in order to come more closely to grips with the problem of the development of skatole, since tryptophane is the parent substance from which skatole is derived. On two occasions this chemist developed acute catatonic schizophrenic symptoms which lasted for some days and required hospitalization. It is presumed that his disturbance occurred, as in the case of Sherwood, from the odoriferous breakdown products of tryptophane he had been inhaling. Perri (1957) has been pursuing this problem further, and finds that tryptophane undergoes many ill-defined oxidative changes upon irradiation as well as upon heating. When submitted to high voltage Roentgen rays, 80 per cent. remains as tryptophane, 7 per cent. goes to identifiable compounds such as indole acetic acid, indole pyruvic acid, etc., and 13 per cent. goes to unknown compounds among which is a large yield of red-brown pigment.

Shortly before his untimely and tragic death three months ago, Sherwood (1957) came to the conclusion that he had found a similar pigment in schizophrenic urine. He believed it to be N-hydroxy-indoleacetic acid, and he was in the process of pursuing this idea more fully when his death came. He found, as did E. L. Ross (1913a) many years ago, that urines of patients suffering from dementia praecox contained significantly more indole acetic acid than did urines of normal persons or those suffering from other mental diseases. This significant observation had been relegated to almost a half century of oblivion because of the belief that this substance was manufactured by intestinal bacteria,

and that its increased level of excretion in Ross' patients was due to the fact that they were chronically constipated. The weight of evidence today is against this notion, and supports the contrary idea that indole acetic acid is derived from the endogenous metabolism of tryptophane, as Ross (1913b) contended originally.

Sherwood confirmed Ross' finding by chromatographic analysis of more than 1,000 urines from normals and from mental patients from two California hospital populations. His findings may be tabulated as follows:

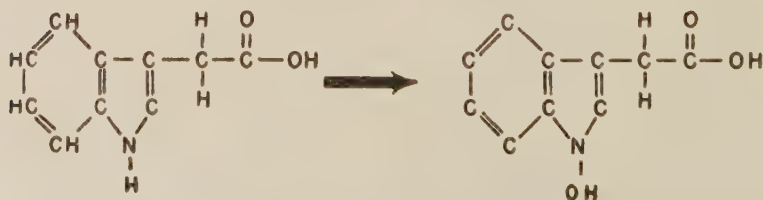
Excretion of Indole Acetic Acid by Normal Humans and Patients with Various Diseases (Sherwood)

Type of Patient	IAA Expressed in mg./litre Urine
Normal adults	2.0-5.0
Stress diseases (allergies, trauma, etc.)	1-1.2
Growth (carcinoma, pregnancy)	1-1.2
Terminal cancer and/or radiation sickness	more than 8.0
Phenylketonuria	more than 8.0
Schizophrenia	more than 8.0

The lowered IAA content of the urine of patients with stress diseases was of interest to Sherwood because a similar lowering can be obtained by giving normal persons cortisone intravenously or orally. He suspected that the cortisone secreted by patients suffering from stress diseases alters the metabolic pathway of tryptophane to IAA. He was interested in the disorders characterized by rapid growth, namely pregnancy and malignancy, as well. The plant physiologists (Skoog, 1950) have found indole acetic acid to be a hormone, or auxin, and Sherwood felt that rapid growth in animal tissues required large quantities of this substance, too, and that they used it up to the extent that little of it was left over to spill out in the urine. When patients with malignancies were near death, however, their tumours no longer took up IAA, he thought, and then it spilled out in the urine in large quantities.

His chief interest in this substance, however, was in the case of schizophrenia. He wrote "Although we have listed schizophrenia among the diseases in which massive urinary excretion of IAA occurs, we believe that there may be an important difference between schizophrenic IAA and ordinary IAA. If one does, as we have done repeatedly, a regular ether extraction of schizophrenic urine at pH₄, one gets out a reasonably high amount of IAA. The same urine, however, turns crimson upon addition of acid to pH₁. It is N-OH indole acetic acid . . . One must not get the impression from the above that indole

SHERWOOD'S SURMISE



DONCCNACATEDELIH -HYDROXY-INDOLEACETIC ACID

FIG. 3.

acetic acid, or its N-OH derivative, is the only substance in the urines of schizophrenics. As all of us know, who have handled specimens of these urines, they are almost as heavily laden with malodorous phenyl compounds as are the urines of phenylketonurics. This is also true of the urines of terminal cancer patients and/or cancer patients suffering from radiation sickness. In all three instances, there appears to exist a massive derangement of metabolism of all of the aromatic amino acids. One common factor to all is the excretion of extraordinary amounts of indole acetic acid."

This brings us to the last area of inquiry concerning biochemistry and schizophrenia—the urinary constituents. In addition to Sherwood's confirmation and extension of Ross' earlier observation, fourteen investigators have found various kinds of differences in the excretion of aromatic compounds in schizophrenics and in the normal population. Of these, the recent studies of McGeer *et al.* (1956a, b) deserve special mention. Using a paper chromatographic technique they found considerable day-to-day variation in the excretion of aromatic compounds in all humans tested, whether normals or schizophrenics. To get round this fact, they studied pooled specimens of urine from normals, schizophrenics, and other psychotics. The urine pools were adjusted to a common specific gravity (1.023). Extracts were prepared by adsorption of aromatic metabolites on charcoal, followed by elution with aqueous phenol, and concentration of the eluate with riddance of the phenol by evaporation, after the method of Dalglish (1955).

Chromatograms of pools of urine extract from acute schizophrenics and from "debilitated" chronic schizophrenics (those too sick to be housed on an open ward) showed at least ten spots which did not appear in the chromatograms of pooled extracts from normal urines. These spots seemed to correspond to abnormal aromatic compounds which either do not appear in normal urine or which are present in imperceptibly low concentrations. Pooled extracts from other types of psychotic patients, chiefly depressives, yielded four spots on chromatographic analysis which did not appear in the normal extracts but did appear in the schizophrenic ones.

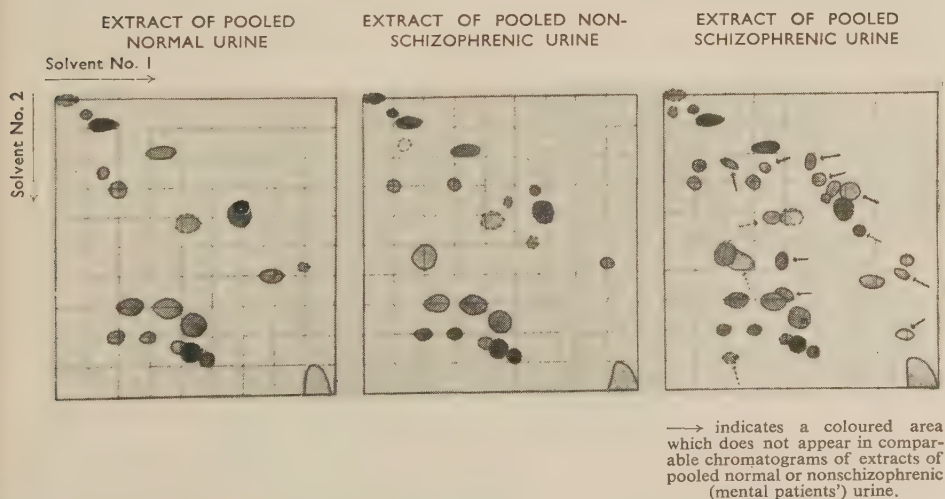


FIG. 4.

SAMPLE CHROMATOGRAMS OF SCHIZOPHRENIC AND
NONSCHIZOPHRENIC URINES

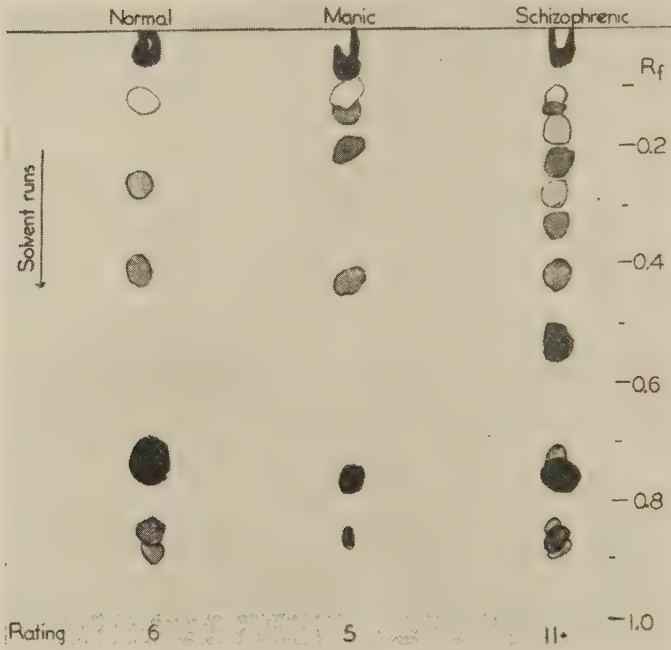


FIG. 5.

The University of British Columbia group of investigators is now at work trying to identify the abnormal compounds which McGeer and McGeer have captured on their chromatograms. Meanwhile, these extracts have been subjected to biological study. Wada (1957) has injected them intracisternally or intraventricularly into cats and monkeys. In cats rage reactions, running reactions, withdrawal reactions and what appeared to be fear reactions resulted from schizophrenic extracts, whereas cats injected with normal extracts did not exhibit these changes. In some of the monkeys a catatonic state lasting for five to eight hours developed following intraventricular or intracisternal injection of schizophrenic extracts, but not in the case of normal extracts. In both cats and monkeys such injection of schizophrenic extracts produced EEG changes in cortical and subcortical leads which were not present when normal extracts were used.

Winter (1957) has made preliminary observations on the effect of intra-peritoneal injection of the McGeer extracts on his rope-climbing rats. Preliminary findings are to the effect that the rats injected with schizophrenic extracts stumble and fumble and cannot climb the rope quickly, and they behave oddly while they are trying to do so, whereas those injected with normal extracts climb it with their accustomed speed and precision.

Much more might be said. Hoffer (1957) has succeeded in isolating adrenochrome in a stable crystalline form, and is pursuing his studies of this adrenaline metabolite and its possible role in the aetiology of schizophrenia. The Tulane group is pressing forward in their study of taraxenin and its mode

of action. Akerfeldt (1957c) is enlarging his inquiry into the chemical dynamics of the oxidation of colourless N-N-diphenylenediamine by various kinds of sera, including the schizophrenic, to a red compound. Riegelhaupt (1956, 1957) is probing more deeply into the reason why the urine of many schizophrenics produces a red-violet colour in the presence of glyoxylic acid and sulphuric acid. He is on the trail of an indole compound which seems to provoke the colour reaction. These are by no means all the people who are at work on related problems.

Is it likely that this will all lead to a single lightning-flash of understanding which will solve the riddle of schizophrenia? It would not take a crystal ball to predict that the answer will not come about in such a fashion. Every clinician senses that we are not dealing with a single disease process in this illness. Eugen Bleuler, the father of the name of this disorder, implied this point of view in the very title of his masterly monograph on the subject. He labelled it, *Dementia Praecox, or the Group of Schizophrenias*. The key word here is the word *group*. Because we are dealing with a group of diseases, it is more likely that our knowledge about exact aetiology will come in parts, and perhaps even in bits and pieces.

When this knowledge comes (and I feel confident that it will, now that so many eyes and so many new tools of inquiry are being put to work), it is probable that Bleuler's term, which has served us so well and so long, will fade out of existence and will be replaced by newer, more precise nosological entities based on more exact aetiologies. Think back a hundred years to a perhaps happier Mid-Victorian time. Many a discourse was given from this historic rostrum about the fevers. At the time these terms served the members of this great Society of Medicine well, and helped them in their understanding of the sick. But as Victoria's long reign drew slowly towards its close, and as the latter half of the nineteenth century unfolded, the bacteriologists came into being. With their guidance, the fevers ceased to exist as they became more exact entities, the infectious diseases.

Will schizophrenia bend and break into its component aetiologies during the second half of our century as did the fevers during Victoria's century, on this occasion under the intellectual weight of a new breed of medical scientists, the biochemists? And will a more rational group of therapies, based on the aetiologies they discover, be forthcoming for these unfortunate sick? The straws blowing in the wind give me confidence that these things will come to pass.

REFERENCES

- ABOOD, L., Brain Research Inst. Conf., Chicago, Ill., 1957a, 12 January, p. 12.
Idem, "Biochemistry and Mental Illness", 1957b. Read at Conf. on Biochemistry and Mental Illness, Univ. Brit. Col., Vancouver, B.C., 20 June, 1957.
 AKERFELDT, S., *Science*, 1957a, **125**, 117.
Idem, Brain Research Inst. Conf., Chicago, Ill., 1957b, 12 January, p. 7.
Idem, Read at 113th Meeting, A.P.A., Chicago, Ill., 1957c.
 ALTSCHULE, M. D., SIEGEL, E. P., and HENNEMAN, D. H., *Arch. Neur. and Psych.*, 1952, **67**, 64.
 BLEULER, E., *Dementia Praecox or the Group of Schizophrenias*, 1911. English translation (1950) by Zinkin, J. New York: Internat. Press.
 BRODIE, B. B., TOMICH, E. G., KUNTZMAN, R., and SHORE, P. A., *J. Pharmacol. and Exper. Therap.*, 1957, **119**, 481.
 DALGLIESH, C. E., *J. Clin. Path.*, 1955, **8**, 73.
 ERSPAMER, V., *Pharmacol. Rev.*, 1954, **6**, 425.
 FABING, H. D., *Neurology*, 1955a, **5**, 603.
Idem, *J.A.M.A.*, 1955b, **158**, 1461.
Idem, *J. Nerv. and Ment. Dis.*, 1956, **124**, 1.
 FEDOROFF, S., *J. Lab. Clin. Invest.*, 1956, **48**, 55.
 FISCHER, R., *Science*, 1953, **118**, 409.

- FREEDMAN, A. M., and GINSBERG, V., "Exchange Transfusions in Schizophrenic Patients", 1957. Read at Soc. Biol. Psych. Meeting, Atlantic City, N.J.
- FUNKENSTEIN, D. H., GREENBLATT, M., and SOLOMON, H. C., *Amer. J. Psychiat.*, 1952, **108**, 652.
- HEATH, R. G., MARTENS, S., LEACH, B. E., COHEN, M., and FEIGLEY, C. A., "Behavioural Changes in Non-Psychotic Volunteers following the Administration of Taraxein, the Substance obtained from the Serum of Schizophrenic Patients", 1957a. Read at 113th Meeting A.P.A., Chicago, Ill.
- Idem* and ANGEL, C., *Amer. J. Psychiat.*, 1957b, **114**, 14.
- HOFFER, A., OSMOND, H., and SMYTHIES, J., *J. Ment. Sci.*, 1954, **100**, 29.
- Idem*, Personal communication, 1957.
- LAZELL, E. W., and PRINCE, L. H., *U.S. Vet. Bur. Med. Bull.*, 1929, **5**, 40.
- LEACH, B. E., COHEN, M., HEATH, R. G., and MARTENS, S., *A.M.A. Arch. Neur. and Psych.*, 1956, **76**, 635.
- LOVETT DOUST, J. W., HUSDEN, H., and SALNA, M. E., *Nature*, 1956, **178**, 492.
- MCGEER, P. L., MCGEER, E. G., and GIBSON, W. C., *Science*, 1956, **123**, 1029.
- Idem* and BOULDING, J. E., *ibid.*, 1956, **123**, 1078.
- MACHT, D. I., *Cincinnati J. Med.*, 1948, **20**, 616.
- Idem*, *South M.J.*, 1950, **43**, 1049.
- OSMOND, H., *Ann. N.Y. Acad. Sci.*, 1957, **66**, 418.
- PERRI, G., "Irradiation of Tryptophan", 1957. Read at Conf. on Biochemistry and Mental Illness, Univ. of Brit. Col., Vancouver, B.C., 20 June, 1957.
- PFEFFER, A. Z., and PESCOR, M. J., *Arch. Neurol. and Psychiat.*, 1944, **52**, 131.
- PINCUS, G., and HOAGLAND, H., *Amer. J. Psychiat.*, 1950a, **106**, 641.
- Idem*, *ibid.*, 1950b, **106**, 651.
- REITER, P. J., *Zischr. f. d. ges. Neurol. u. Psychiat.*, 1938, **160**, 598.
- RIEGELHAUPT, L. M., *J. Nerv. and Ment. Dis.*, 1956, **123**, 383.
- Idem*, "Indolic Excretion of Schizophrenics", 1957. Read at Conf. on Biochemistry and Mental Illness, Univ. Brit. Col. Vancouver, B.C., 20 June, 1957.
- RINKEL, M., HYDE, R. W., and SOLOMON, H. C., *Dis. Nerv. Syst.* 1954, **15**, 259.
- ROSS, E. L., *Arch. Int. Med.*, 1913a, **12**, 112.
- Idem*, *ibid.*, 1913b, **12**, 231.
- SHERWOOD, W. K., *Trans. Soc. Biol. Psychiat.*, 1956, **10**, 53. Cincinnati: Merrell.
- Idem*, "Urinary Excretion of some Tryptophan Metabolites in Health and certain Diseases", 1957. Unpublished MS.
- SKOOG, F., *et al.*, "Symposium on Plant Growth Substances", 1950. Univ. of Wisconsin Press.
- STOLL, W. A., *Schweiz. Arch. Neurol. u. Psychiat.*, 1947, **600**, 279.
- SULKOWITCH, H., and ALTSCHULE, M. D., *The Excretion of Urinary "Epinephrines"*, in *Psychiatric Disorders*. In press.
- WADA, J., "Behavioural and EEG changes induced by the Injection of an Extract of Schizophrenic Urine", 1957. Read at Meeting of Soc. Biol. Psych., Atlantic City, N.J., June, 1957.
- WINTER, C. A., and FLATAKER, L., *J. Pharmacol. Exper. Therap.*, 1951, **101**, 156.
- Idem*, *Proc. Soc. Exp. Biol. Med.*, 1956, **92**, 285.
- Idem*, "The Effect of Blood Plasma from Psychotic Patients upon the Performance of Trained Rats", 1957. Read at Soc. Biol. Psychiat. Meeting, Atlantic City, N.J.
- YOUNG, H. K., JR., *et al.*, *Univ. of Texas Publ.*, 1951, 5109, 189.

DISCUSSION

By Dr. D. Stafford-Clark

As the British contributor to this meeting with the privilege of opening the discussion of the last paper of the two-day symposium, I would like to precede such observations as I have to make on this most stimulating and interesting paper by echoing what I know is in the hearts and minds of all British members present at this meeting. It is this: We have all at some time or another had occasion to recognize and appreciate the warmth, generosity, and above all the unassuming friendliness and kindness of American hosts, some of whom are included in our guests here today. Many of us have travelled in America, worked there, or enjoyed professional contacts which have placed us in the position of accepting American hospitality. And we cannot have failed to be touched by the sincerity and enthusiasm with which our American friends have set out to entertain us and to make us feel happy with them, and at home in their society. In the past two days it has been our privilege to extend what we hope has been a comparable hospitality, and certainly one motivated by a

comparable enthusiasm and delight in being on this occasion the hosts ourselves. May I say on behalf of all the British doctors present, and the two societies who have jointly arranged this meeting, how delighted we have been to have you, our American guests, with us; how stimulated we have been by what you have told us; and how deeply we hope that you will go back to your own country, taking with you memories as warm, happy, and lasting as those which you in your turn have given us.

In moving to a discussion of the paper we have just had, I have had to make some alterations in the general gist and arrangement of what I had planned to say, because Dr. Sargant has just given us the welcome news that Drs. Humphrey Osmond and Hoffer are present here today, and will both say a few words in the context of this paper.

I shall therefore omit all specific reference to their remarkably stimulating contribution to our knowledge and speculations about the Biochemistry of Schizophrenia, since although this must inevitably form an important aspect of any survey of this work as a whole, they can be left to speak about it for themselves. There remain a number of points whose brief consideration may be relevant and helpful in discussing this topic.

Dr. Fabing was modest enough to say that it might be thought presumptuous of him as a clinician to make noises like a biochemist. I do not entirely share his modesty, and indeed fear that I may even exceed what he would regard as his own presumption; for I intend, being also a clinician, to make some noises like a communication engineer, some noises like a cyberneticist, and even a bashful cheep or two like a mathematician; and in truth I can claim to be none of these things.

In one sense, the cardinal features of schizophrenia may perhaps be regarded as respectively:

1. A failure to make the usual interpretation of external or internal events; for example, to understand what is going on in the world outside, or indeed what might be going on in one's own body.
2. A failure to communicate in the usual way with other people.
3. A failure to integrate interpretation and communication, within oneself, or with others, in a way which will even allow the first two difficulties to become apparent in their full nature to any but the trained observer.

There is in fact, as far as brain function is concerned, a failure of communication and control. This is of course a common feature of breakdown or disruption of function in communication engineering, and is one of the ways in which electronic computers themselves can be described as going wrong. The brilliant and exciting work of Grey Walter and Ross Ashby in this country, and of MacCulloch in the United States, has shown with what relevance we may make models of the brain in terms of various types of electronic computer.

These practical experiments in cybernetics can bridge, to some extent, the gulf separating our knowledge of brain biochemistry and electrophysiology, from our knowledge of actual behaviour. Apart from the elementary insight into the relationship between brain function and behaviour which such models can provide, we remain confined to the conception of brain, in mathematical terms, as a "black box"; that is a system whose detailed nature is unknown, but whose behaviour can be described in terms so formulated as to accept the unknown as a part of the whole system, which does not necessarily interfere with the consistency of that system's external behaviour.

Regarding the brain as a "black box" of this kind, Ashby has pointed

out that one of its characteristics would seem to be that of ultra stability. Ultra stability may be roughly defined as the capacity to change the general field of behaviour in conformity with a change in external circumstances, brought about by the effect of signals from the outside world upon the black box itself, in such a way that the behaviour of the black box remains able within its own limits to deal with every possible variation in signal information, and to make responses which remain in a varying degree appropriate to the external changes which have been imposed.

There is of course a great deal of evidence to support our analogy of the brain in this sense as an electronic computer; albeit an electronic computer of possibly immeasurable complexity, which provides for its remarkable flexibility and wonderful precision. The electroencephalogram, crude though its information must yet remain, at least supports the view that it is in its electrical function that we must seek the practical key to the work of the brain. Its communication is by electrical signals, and their transmission from neurone to neurone through synapses is probably the way in which patterns of activity are regulated and controlled. It is indeed possible to build up a mathematical theory of communication within the brain, using concepts of this kind.

But all models which have so far been constructed have taken the source of their electrical power for granted. Such power has in fact been piped in from the local mains supply in the laboratory, or stored in accumulators hitched on to the working model.

I am sure that we must never forget that the brain is the only electronic computer which manufactures its own electricity biochemically as it goes along. In studying the functions of the brain, therefore, we have to remember that we are at least two removes both from the subjective experiences, and from the output in terms of behaviour, which we seek to explain. At one remove there is the electrical network, in all its wonderful complexity and beautiful intricacy; and at the second remove there is the biochemical process, also wonderfully intricate and many sided, whereby the brain continues to live, to breathe, to gain its nourishment, and to make its power. It is in the biochemical aspects of brain function in health and illness that we see those changes which may affect both the conductivity and the availability of electrical power within the system. And it is the ultimate translation of this power into electrical signals, which are themselves the only measurable accompaniment to thought and feeling, which presumably alone makes these processes possible.

Nevertheless, though we operate necessarily at these two removes, we can see a simple biochemical correlation*beginning to appear in terms of behaviour and experience, which relates the normal stresses of anxiety at one end, to the delusions and hallucinations and disordered thinking of schizophrenia at the other. I have attempted to work this out in another place, in a contribution to the symposium on schizophrenia recently published by a group of British workers under the general title of "Schizophrenia: Somatic Aspects". In briefest outline the theory is this.

The normal adrenergic response to stress includes the subjective feeling of anxiety as one of its components. Another component is the increased concentration of adrenaline in the circulating blood and probably in the brain itself. This has been shown experimentally to affect synaptic transmission within the brain and nervous system, and may well account for the tendency in anxious and apprehensive subjects to misinterpret or misidentify marginally, external experience. The worried sentry, crouching in his foxhole, tends in his accumulated fear and tension to mistake a tree for a crouching enemy,

a creaking bough for the sound of a crawling man. This misinterpretation, amounting to a transitory illusion, is caused by nothing more toxic or abnormal than a high adrenaline concentration.

The next stage towards a pathological condition may be postulated as exhaustion caused by protracted loss of sleep, wherein actual hallucinations and disorder of thinking may be seen symptomatically, as exemplified by observations of volunteers who have taken part in sleep deprivation experiments and subsequent tests. One stage further on, the toxic confusional states, and episodes of delirium show us how exhaustion plus circulating toxins or metabolic errors can produce profound states of mixed psychotic and dementing symptoms, characterized by gross interference with the patient's interpretation of reality. It is but a short step from these to the manifestations of the group of schizophrenias with which we are concerned.

Smythies, Osmond and Hoffer have suggested that we can bridge this gap by a study of the so-called model psychoses which can be brought about by a number of hallucinogens, and of which they will doubtless have something to say themselves. We thus have, as it were, an ascending scale of disordered brain function from apprehension at one end to schizophrenia at the other, biochemically related to and mediated by the effect of circulating chemical substances upon the electrical conductivity and synaptic pattern of brain function.

An interesting sidelight on this is provided by an observation which you may have noticed in the exceedingly interesting work on the climbing performance of rats, quoted by Dr. Fabing in his paper. May I remind you that of the 14 groups of rats tested, the only group whose speed of performance in climbing was *greater* than that of the normal group, was the group of rats (No. 11) treated with serum from emotionally unstable patients? Their average climbing time delay was shown by an index of 67, less than half that of the rats who had received injections of serum from volunteers in normal health. If this can be taken as indicating anything significant at all, it surely indicates that emotional tension may actually facilitate speed of reaction, and increase the capacity for this *above that normally existent*. It is only when such apprehension passes over into chronic exhaustion, toxic confusion, or the biochemical disturbances of schizophrenia, that the reverse and pathological effect upon speed and facility of response are produced. But I repeat, there is evidence that these are all stages along the same ascending scale of biochemical interference.

It remains important to remember that in studying brain function we must never allow our relative technical proficiency in measuring biochemical changes to blind us to the fact that these are of ultimate importance only in their effect upon patterns of electrical activity, conduction, communication, and control in the electrical functioning of the brain; but as clinicians we must not forget that communication remains the ultimate key to the treatment of schizophrenia, or indeed of any other kind of illness.

Clinically, it is the failure of communication and control which brings the schizophrenic to the doctor. However skilful and appropriate our physical treatment of schizophrenia may become as the result of refinements in our knowledge and understanding both of the biochemical processes which may underly it, and the electrophysiological processes whose secondary disturbance ultimately brings about the syndrome, it remains true that we can only treat patients successfully, and restore them to true health and happiness, if we gain contact with them at a human and personal level, and give them

thereby the bridge over which they may cross back into normal harmony and understanding with their fellows. Both Dr. Fabing and I admitted at the outset that we spoke as clinicians; and it should be the badge of the clinician that he deals with his fellow human beings, whether at the bedside or in the consulting room, and whatever the nature of the sickness which afflicts them, in relation to their whole life and happiness. The foundation of clinical work must remain contact between doctor and patient; contact to which clinical psychiatry deservedly pays great attention, and may further specify and define in terms of rapport. Whether we regard this as the harmonious and flexible communication between two systems, symbolized by two separate black boxes, or whether we regard it in philosophical terms as a universe of discourse between two minds, it remains true that it is upon the flexibility, imaginative sympathy and understanding of the doctor that the control of the communication system, and so in the long run the security of whatever life-line can be extended to the patient, must ultimately depend.

Reviews

Sigmund Freud. Life and Work. Volume Three. The Last Phase. 1919-1939. By ERNEST JONES. The Hogarth Press, London, 1957. Pp. 536. Price 35s.

In this last volume, published just before his death, Dr. Ernest Jones writes objectively with knowledge, ability and love to give the world a worthy account of Sigmund Freud, his life and work.

Ernest Jones was always to be relied upon to give Freud tactful advice and endless help. When all was not well amongst colleagues he wrote to Freud, "You know that it is essentially for you that we are all working, which is why your inspiration and approval mean so much to us all."

Jones also toiled hard for the escape of "colleagues who were more than ostracized in Berlin"; and, after the Nazi invasion of Austria in 1938, he engineered the very tricky evacuation of Freud from Vienna to London.

This third volume is best epitomized by the heading of the fourth chapter—"Fame and Suffering"—this thought haunts one even when reading the excellent historical reviews by Jones on Freud's contributions to, for example, Metapsychology, Biology, Anthropology and Religion.

The story of Freud's life is resumed in 1919 with the coming together of leading psychoanalysts to form a Committee of six. Two of these six subsequently "developed psychotic manifestations"—one can only comment that if this proportion is statistically significant it underlines the cost by which humanity gains its knowledge. Such thoughts are still more prominent in "the epic story of Freud's suffering". In 1923, cancer of the jaw being diagnosed, Freud had the first of the thirty-three operations he underwent before he ultimately died in 1939. He endured all this without complaint and without the help of religion. He did indeed have the help of his daughter Anna who nursed him from the onset of his illness to the end of his life, but the pact they kept was that, however agonizing the situation, "no sentiment was to be displayed".

"But with all this agony there was never the slightest sign of impatience or irritability. The philosophy of resignation and the acceptance of unalterable reality triumphed throughout."

It was against this background that fame came to Freud. He was aware of the drawbacks to fame and even before his long last illness his reply to a letter congratulating him on the progress psycho-analysis was making in the world was "... you seem to overestimate my enjoyment of it. What personal gratification is to be drawn from psycho-analysis I had already enjoyed in the time when I was alone, and I have been more annoyed than pleased since others have joined me." Jones says, "Above individual friendships Freud had come to treasure still more the value of his discoveries and all that ensued from them."

As all the world knows, his theme was sexuality and Freud insisted on its being recognized "that that is our Shibboleth".

Jones says, "It is easy to state the parts of his work to which Freud attached the highest importance . . . They were (1) the seventh chapter of *The Interpretation of Dreams* (1900), (2) the last chapter of *Totem and Taboo* (1913), and (3) the essay on 'The Unconscious' in his metapsychological series (1915). They amount altogether to only 220 pages (!)". Apparently Freud thought his "The Future of an Illusion" to be of "very little value"—saying of it, "I regard it as weak analytically and inadequate as a self-confession."

Jones in retrospect assesses soberly Freud's influence on human knowledge and life. Thus Freud's influence on Social Life includes the fact that "mistakes in daily life, the forgettings, mislayings, slips of the tongue and pen and so on . . . are no

longer always passed by as 'accidental' . . . Most important, however, is the increasing sense people have of being moved by obscure forces within themselves which they are unable to define . . . and this . . . we owe above all to Freud."

C. E. HEDGMAN TURNER.

The Shakespearean Ciphers Examined. By WILLIAM F. FRIEDMAN and ELIZABETH S. FRIEDMAN. The University Press, Cambridge, 1957. Pp. 303. Price 25s.

The arguments on the Baconian controversy run the gamut from sense to nonsense. Let me say at once that this book is written by two experts on the subject of ciphers, professional cryptologists in the U.S. military and diplomatic services; they take the various ciphers that have been proposed as existing in the Shakespearean texts and study them exhaustively.

The book is first-rate. It is masterly, impartial, and clearly written. It demonstrates beyond question that the so-called ciphers are not ciphers in any proper sense, but "rules" with so many exceptions and variations that, as the authors demonstrate abundantly, almost any text may be made to yield almost any message. The book can first be recommended simply as an interesting study in applied cryptology. However, it has psychiatric applications. The subject clearly lies on the fringe of paranoia, and the authors' study of how Donnelly, Gallup, Owen, and many others have thought and "reasoned" may give, to those interested in paranoia, an insight into those mental processes that are almost within normal limits yet not fully psychotic. Contrary to a psychiatrist's expectation, most of the cipher-proposers are not psychotic in any ordinary sense; and it is this fact that makes the book unusually interesting.

Mrs. Gallup, for instance, attempted to apply a cipher that Bacon is known to have proposed; she found that after a little coaxing, and by making some allowance for compositor's errors, some sort of message began to emerge. She admitted that she was not sure what credence to give it, but as page after page of the first Folio continued the "message", she felt unable to reject it. The Friedmans showed, by experiments on each other with related ciphers, that the actual freedom allowed by her in the interpretation of the letter forms was, in fact, sufficient so that the growing "message" would never break down irremediably into nonsense. Though the breakdown was incessantly threatened, there was always enough freedom for further adjustments, perhaps by going back a little way for a fresh line, so that a sufficient degree of "sense" could be preserved. What the authors demonstrate is how little freedom will suffice to make the threatening chaos always at least postponable. Mrs. Gallup was probably an innocent victim of this not very obvious property of ciphers and their breaking. The others, however, come nearer to the psychiatric domain.

Those who are interested in the psychology of paranoia may well find this book illuminating and suggestive. It will repay detailed study.

W. ROSS ASHBY.

Schizophrenia: Somatic Aspects. By nine contributors, edited by D. RICHTER. Pergamon Press, London, 1957. Pp. 181. Price 40s.

This book is very simple to review, for it is an up-to-date account of our knowledge about the somatic aspects of schizophrenia, each aspect reviewed by a well-known expert in the field. The whole subject is treated adequately, without being overloaded with insignificant detail; as a result, every part is readable. Paper and binding are good, and the print is beautifully clear.

Those who want to remain well informed on the subject of schizophrenia (and who does not?) cannot afford to overlook it.

W. ROSS ASHBY.

An Introduction to Cybernetics. By W. ROSS ASHBY. Chapman and Hall, Ltd., London, 1956.

Dr. Ashby, who has become well known over two decades of writing in the field now known as cybernetics, has ventured the difficult task of writing an intro-

ductory textbook in a new and rapidly developing area with many facets. Intended primarily for biologists and specialists in related fields such as neurologists, neurophysiologists, psychologists, psychiatrists, and social scientists, he boldly offers a minimal mathematical treatment of subjects which are basically mathematical in nature. He presents a step by step exposition of the fundamental theorems of cybernetics, principally from biological systems displaying machine-like behaviour. Formal definitions of the customary basic concepts such as system, variable, variety, constraint, environment, adaptation, bit, equilibrium, entropy, channel, capacity, noise, regulation, coupling, amplification are clearly given with numerous illustrative examples so that the beginner may welcome this book for its reference value alone. The presentation of the problem of the "Black Box" (pp. 86-120) from its beginnings in electrical engineering to current applications to biological models and isomorphism is a good example of the author's skill in orderly exposition even though the reader may occasionally question both the logic and the content. His hopes for the topological approach in psychology, especially that published under the pseudonym of Nicholas Bourbaki (a group of Frenchmen described in the *Scientific American*, May, 1957) may be premature or even exaggerated, but deserve attention. It is also important to note that throughout the book there are strategic cautions placed against misuse of the theorems, as in the discussion of Shannon's measure of entropy (p. 176), and the warning against extravagant expectations in the study of very large systems (p. 245). Possibly the discussion of co-ordination, regulation and control and the new account of the principle of "ultrastability" in Part III will be the most stimulating to both the beginner and expert. The overlap with *Design for a Brain* (1952) is principally in this section, but the two books are "almost independent", and can be complementary. There is nothing new about the "homeostat", the machine that can learn.

The orientation of the author is the familiar functional, behaviouristic inquiry in the ways of behaving rather than a concern with things. He asserts the independence of cybernetic principles from other branches of sciences, and uses geometry, including non-Euclidean forms, as a prototype. "Cybernetics stands to the real machine—electronic, mechanical, neural, or economic—much as geometry stands to a real object in our terrestrial space" (p. 2). Cybernetics is conceived as related to information theory proper because it is concerned with sets of possibilities in which the concept of energy is not necessary or relevant. Of the many uses of these concepts to biologists, he cites two generalizations as of paramount importance. "One is that it offers a single vocabulary and a single set of concepts suitable for representing the most diverse types of systems" (p. 4). He expresses the hope that here, as in the past with other sciences, progress in one field will assist development in another. "The second peculiar virtue of cybernetics is that it offers a method for the scientific treatment of the system in which complexity is outstanding and too important to be ignored" (p. 4). Only time will tell whether these hopes will be fulfilled in the biological field in the particular sense in which they are presented, but it is the latter thesis which is of particular interest to biologists because of our current difficulties in dealing with the very large systems. Of course, the presentation cannot be read as a cure-all, for it offers only potential tools by means of a modest exposition of principles to beginners. Of this field it has been said that direct study of the brain and its functions would be a more profitable way to explore its activities.

There are other criticisms aside from the fact that this book might mislead the naïve into unwarranted enthusiasms. Because science is a continuing process in which the detection and self-correction of error is an essential feature, it seems probable that correction or reconciliation of conflicting views will be effected by those practitioners most concerned. The following questions have been raised and answers will probably be provided in the due course of time. Is it possible to do justice to the full scope and power of an essentially mathematical subject such as cybernetics using only minimal mathematics? Is not an over-simplified mathematical treatment always bound to be more or less incomplete, and perhaps, even misleading? Does this book prepare the student for reading more advanced technical works? Are the examples provided for

working out problems sufficiently rewarding to be worth the effort? Is it possible to master the principles of cybernetics without some first-hand knowledge of electronics as well as advanced mathematics?

Other opinions are that some concepts are not used in a conventional manner, including the definitions of cybernetics, vectors, feedback, probability, and that some generalizations regarding memory, emergent properties, and "information-tight" systems are not justified or are treated in an arbitrary manner. Is the exposition of discrete states disproportionately long with consequent relative neglect of the important continuous states? It is true that the author does not present the reader with an experimental model of mind which is immediately useful in the laboratory, nor does he discuss many aspects of mental activity and the adaptations involved, but this can hardly be expected at this time.

In spite of the foregoing questions, this reviewer found the study of this book worthwhile because of its lucid exposition of many vexing logical and technical propositions. The rich content and potentialities are a liberating influence in thinking about biological systems even though its practical importance to psychologists and psychiatrists remains to be demonstrated. The colourful examples drawn from many fields are often conceived with ingenuity and humour. Granted that the working out of these examples is an exercise which will not solve either fundamental questions nor practical problems, and that they require a degree of attention most of us do not easily devote to other than serious work, it can be said in their favour that they compel more than a cursory reading of a complex subject.

Unfortunately for clinicians, Dr. Ashby has not included some of his better clinical material from earlier writings, particularly the application of cybernetics to psychiatry published in this journal (100, 114-124, 1954). Paragraphs about causalgia (p. 82), cough reflex (p. 247), sensory and motor restriction (p. 220) are short, and the words neurosis and psychosis do not appear at all, but this was not intended to be a textbook on psychopathology. Although this introductory text does not meet all needs, it deserves a place on the shelf of the student interested in new ways of thinking about human behaviour because of its wide scope, clear writing and picturesque examples.

HENRY W. BROSLIN.